

paul ehrlich:
three cheers
for technology

the battle
of the weapons labs



THE
bulletin

OF THE ATOMIC SCIENTISTS

a magazine of science and public affairs



September 1975

\$1.50

**nuclear
reactor safety:
a special
report**



WHY IS NUCLEAR TECHNOLOGY FOR SALE?

Canada is preparing to sell a nuclear reactor and its accompanying technology to South Korea. Arguments about the instability of nations and the insecurity of nuclear safeguards have fallen on deaf ears in those countries, as in others. There are too few with the necessary moral weight, political leverage, or technical knowledge to warn of the dangers of such trade.

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THIS ONE



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LETTERS

No Sacred Cows

In a nine-line letter to the editor (*Bulletin*, May 1975), Carl Dunbar qualifies himself as a reader of the *Bulletin* since it began, calls George Kolodiy a "joker," protests your publishing his article with "the ghost of Velikovsky," and assumes that "since the disappearance of Dr. Rabinowitch the current editor is not a scientist." In a three-line response the current editor says that he "is a journalist, as was the late scientist editor-in-chief, Eugene Rabinowitch." This diminutive exchange is important because of the story of the scientific and popular controversy from which it sprang.

The well-known and often bitter Velikovsky affair has propagated a whole spectrum of waves of light and heat. Some great scientists, led by the late astronomer Harlow Shapley, denounced and boycotted Velikovsky—often concededly unread—calling him a crank and a liar. Others, while respecting his industry and his imagination, laughed at his celestial collisions as impossibilities. Still others—and I am of these—suspect that some at least of his startling views may prove correct, but are given pause by many doubts and objections, such as the chronology of Stonehenge, for example, or the recent theory of continental drift with its own catastrophic effects. And of course there are the fanatic disciples who treat Velikovsky as a latterday saint whose every opinion is to be held sacred.

Each reader may choose his own place in this spectrum. But—what is far more important—each reader should, I believe, support Velikovsky's access, until he is proved wrong, to the best scientific journals and to truly impartial conferences and to the help he has sought so long and so vainly for the testing of his theories. His remarkable successes in predicting new scientific discoveries surely entitle him to this. No natural law, not even celestial mechanics, can any longer claim immunity from his thoughtful questioning. I applaud the *Bulletin's* interest in this subject.

Bertram F. Willcox

Professor Emeritus
Cornell Law School
Ithaca, N.Y.

Obscene Definition

I have just seen the article, "Peace: A Subject Worth Studying," by Matthew

Melko (*Bulletin*, April 1975). On looking at its title entitled "Peaceful Societies in Recent History," and seeing the British listed as a peaceful society from 1485 to 1940, I had a bit of difficulty believing that my eyes had not failed me. This includes Britain's imperial period, the Napoleonic Wars, Britain's armies fighting in North America, India, Egypt, the Sudan, South and Central Africa, heaven alone knows where not. The miracles that 'scholars' can produce with phony categories are truly endless. Going to the text to see how this particular miracle was performed, one finds the author writing:

To begin with, I shall consider peace to be an absence of war, revolution or other physical conflicts among men. . . . Sometimes a country, like Britain in the eighteenth century, participates in a series of external wars, while maintaining peace at home. . . . So if a region or state is peaceful, I shall consider it peaceful regardless of what its government may be doing somewhere else.

This is simply beyond belief. If anyone has ever cooked up a more ridiculous, almost obscene definition of 'peaceful' or of a 'peaceful society,' I don't know that I've ever come across it. Even our Department of Defense with its years of experience in concocting phony categorizations and descriptors could not dream up this one.

I then wondered if the author would include the United States, as we only slaughtered a million or so Filipinos between 1896 and 1905 or thereabouts, and another million Indochinese from 1965 to 1972. And, sure enough, there was the United States listed in the table as a peaceful society from 1866 on, with a minor remark, "interrupted by sporadic conflicts with indigenous inhabitants in 19th century," that is, the million or so American Indians we also slaughtered along the way.

There's nothing like a good definition to make a good table. I recently saw another prime example of this genre, the RAND report, *International Terrorism, A Chronology, 1968-1974* (B. M. Jenkins and J. Johnson, R-1597-005 ARPA, March 1974). In the sections on Latin America and the countries of Argentina, Brazil and Guatemala, there is not a single word or mention of the killings of literally thousands of individuals in each of these three countries by semi-official moonlighting police and military groups. These are not defined as "terrorists."



Only the left wing groups are listed. In the section on Europe, there is no discussion of Italy, in which nearly all of the terrorist groups are right wing.

In the RAND report

Only incidents that had clear international repercussions were included—incidents in which terrorists went abroad to strike their targets, selected victims or targets that had connections with a foreign state (e.g., diplomats, foreign businessmen, offices of foreign corporations), or created international incidents by attacking airline passengers, personnel and equipment.

Thus again, the problem is not that a definition was not presented, but that the categories are contrived to omit by far the greater segments of the kinds of events they ostensibly pretend to have under discussion. Melko's 'peaceful' counted only killings at home, while RAND's 'terrorism' counted only events away from home.

Well, RAND is RAND, but spare us this kind of scholarship from "peace researchers."

Milton Leitenberg

Peace Studies Program
Center for International Studies
Cornell University

The author replies: Milton Leitenberg's response to my article is quite understandable, and I am grateful to him for putting the matter so eloquently and succinctly. The matter has been raised many times by scholars who have had the opportunity to see the data and Britain is usually the case that draws the most incredulity.

My approach was to look for peace where I could find it. Now when J. David Singer and David Small look for wars, they do not ask whether the governments involved might not be preserving peace somewhere else (*The Wages of War 1816-1865* New York: Wiley). So, when I looked for peace I did not require that the government of that society be at peace everywhere else. After the areas of peace were established, then I thought it would be proper to examine



the causes. The case of New Zealand's high and seemingly unnecessary casualty rate in three external wars made me wonder if it could be that peace could be preserved only by sending your young men off to kill somebody else.

When the question was investigated, however, I found that the governments of 20 of the 51 societies were not involved in any external conflicts during their peaceful periods. Of the remaining 31, only 12 (the British being one) were involved in conflicts that were clearly unnecessary for defense (Melko, 52 *Peaceful Societies* (Oakville, Ont.: Canadian Peace Research Institute, 1973), pp. 78-79, 100-101).

Certainly there are other ways of defining peace. I hope that Milton Leitenberg will be moved to devote some of his research time to developing a better definition of peace and, out of that, a better set of cases. Peace is a subject worth studying.

Matthew Melko

Bradford College
Haverhill, Mass.

Peaceful England?

I enjoyed Matthew Melko's study of peaceful societies (*Bulletin*, April 1975), but it may be necessary to cross 18th Century England off the list. This was the golden age of the London Mob; demagogues of the day had riot management knocked down to a system. Benjamin Franklin, who lived in England for some years, reported "riots about corn, riots about elections, riots of colliers, riots of coal-heavers, riots of sawyers, riots of Wilkesites, riots of chairmen, riots of smugglers."

George III frequently got treated to a pelting of rocks and rotten tomatoes when appearing in public. Preindustrial social conditions being what they were, rioting virtually became the national participant sport of the lower classes. Domestic tranquility was not a strong point.

Compared to England, the American

colonies—even Boston, which was sort of the Dodge City of the British Empire—were positively peaceful. The Bostonians never raised riot engineering to the status of a fine art, as was the case in 18th Century English urban centers.

James J. Glackin

Pasadena, Calif

Enough, Enough, Enough

It now may be stated categorically that the United States possesses far more than enough nuclear weapons of every kind which might conceivably be needed to meet potential military needs for the next 100 years—even possibly for all time. The same may be said for the Soviet Union.

Yet the utterly insane nuclear arms race continues as though each nation yearns for reciprocal suicide as a means of solving all lesser problems. It will at least do that easily enough—only a touch of the button is needed.

Notwithstanding this, our Pentagon gluttons cry for more, and the more yardage of colorful ribbons they carry on their sagging and lopsided left breasts, the more they cry for more.

But it is truly time to stop. Admittedly, the military and/or the diplomats because of their inbred and natural peculiarities can not and will not do it. But the Congress of the United States—that sometimes faltering but nevertheless unique embodiment of our whole system of democracy—can, should and must do it. The method is simplicity itself. Congress should utter *enough, enough, enough*, and trumpet it so loudly that it will be heard around the world. It will not be recognized as a confession of weakness on our part but as ringing affirmation of the realization of our titanic strength and our courage to recognize it. The Soviet Union, now wearying its people as much as ours are being wearied with the insanity of the arms race, will also be glad to say *enough* when our credibility is restored.

Three simple, unilateral acts by the U.S. Congress will do it:

First, Congress should unilaterally order the immediate and total stoppage, by or for the United States, of any and all forms of nuclear weapons and their launchers; and reject any appropriation, however concealed, for their further production, testing, repair, or improvement in any manner.

Second, scientists, technicians, workers and others now working on these

weapons should not bear the brunt of their unemployment. Congress should provide that they are paid their full salaries until they are reemployed or retired, thus conserving vital purchasing power for themselves and the nation. As these salaries are about 60 percent of the arms cost, the budget saves 40 percent.

Third, Congress should establish a great commission—independent and powerful as it can be made—to see that the Congressional mandate of *enough* is obeyed in letter and spirit, so that this nation's sadly eroded credibility be restored.

One thing only remains to be done: a steadily increasing call of *enough, enough, enough* by the people to their representatives in Congress, rising in crescendo until Congress hears it clearly and responds.

Willis L. Hotchkiss

Des Plaines, Ill.

Dramatization vs. Responsibility

The question of migration of scientists, particularly when they come from a country like the Soviet Union, is a politically subtle and complex one. This and other considerations attempting an even analysis of the phenomenon of Soviet scholars' migratory movement were contained in my article, "Problems of Scientists' Migration: The Soviet Emigration of the '70s," which I sent to the *Bulletin*. However, when the article appeared in the June 1975 issue, I found it imperative to dissociate myself from that publication. First, because a different and totally misleading title had been substituted; second, because the edited version published had not been agreed upon by me. Finally, the article was most inappropriately and vulgarly illustrated.

In the article I analyzed expectations of the emigrating scientists and modes of their eventual fulfillment. I also pointed out that the scientific community in the West, probably the most active single professional group to support their Soviet colleagues' plea for emigration, may have a more substantial say as to the émigrés' resettlement in a new milieu. The published version which the editor dramatically entitled "Soviet Jews: The Bitter Aftermath of Emigration," distorted both the objective of my article and my own viewpoint.

Were it only a personal affront, a less conspicuous method of assuaging the

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consequent ills might have been chosen. However, the issue is more important than myself. It involves the aspirations of countless numbers of people. By abusing its editorial powers in this fashion, any journal, and more so one as reputable as the *Bulletin*, jeopardizes not only the efforts of those outside the USSR to humanize the exit, but it also hampers the movement inside the Soviet Union. Hopefully, something will be learned from this experience which will preclude the possibility of a "bitter aftermath."

Yakov M. Rabkin

Institute for History of Science and
Science Policy Studies
University of Montreal

Editor's response: The edited manuscript was approved by the author six weeks before publication. The title and illustration were supplied by the *Bulletin* in the belief that they were appropriate to the article. While we do not accept the author's characterization of the title and illustration, we do regret that our efforts to help him present his ideas have offended him.

Hard to Understand

I have been a subscriber to the *Bulletin* for the past several years, but I find that it has become increasingly irrelevant in helping to form my ideas on the important issues it discusses. What I find particularly difficult to understand is the apparent glorification in the *Bulletin* (the All In Our Time series) of the men who designed and built the Hiroshima and Nagasaki bombs while portraying a consistent bias against those who are trying to develop commercial nuclear power.

I regret that I will not be able to renew my subscription to the *Bulletin*.

Thomas E. Murley

Bethesda, Md.

The Lost Sheep

All of us, your readers, have been tormented in the past few months by your appeals for help, fearing that you may fold. It would be exceedingly sad if such a brave venture should disappear like all the others. But some of us, or at least the undersigned, suspect we know why you are in such difficulties.

Take the April 1975 issue which, because of seismal, is the most recent I have read. Samuel H. Day, Jr., identified on your masthead as your editor, pretends that we could make the problems of war or threatened war disappear by kidding the armed services that pollution is a bigger threat. Albert Szent-Györgyi (who should know better) talks of the

disappearance of man (whoever he is) before the end of this century. Ruth Sivard tells us that the world would be a better place if governments spent what they spend on arms on other things, but provides no analysis of their waywardness nor even a suggestion as to how they might be persuaded to change their ways. Anne Cahn explains that the arms trade is growing but that the customers are being provided with better weapons than those available a few years ago. Kosta Tsipis comes out against the long-range cruise missile without saying much about its vulnerability to countermeasures or even explaining why Ruth Sivard should not welcome a cheaper road to over-kill.

As journalism, it is monotonous. As a contribution to the debate about arms and arms control, it is one-sided. As a means of provoking creative thought about the ways in which disarmament might be brought about, it is at once too qualitative and too little concerned with the stubborn political realities of the world we live in.

It is my impression that the message your founders had to tell was compelling because their alarm at the prospects of nuclear war was tempered by their belief that something could be done about it. Perhaps I was wrong, but if not, I hope you will change.

John Maddox

London, England

Editor's response: Regrettably, the writer furnished no instructions in being less monotonous, thinking creative thoughts or understanding "the stubborn political realities of the world" so that we might be guided back to the spirit of our founders. Thus, we are left to grope for examples, such as the following, which he provided in an article, "No Need for NPT Gloom," in the June 19, 1975 issue of *New Scientist*: "On balance, then, to pragmatists, the (Non-Proliferation Treaty) Review Conference has done some useful work. Almost by accident, it has helped to identify the most serious impediments to further progress in arms control. Nuclear-free zones have a new importance. The dilemma of peaceful nuclear explosions has been exposed. The need to induce both France and China to join the NPT system . . . has been made plain for all to see. The NPT itself has survived without the defection of a single member. And there is to be another review conference five years from now—an interval of time during which it is exceedingly unlikely that any of the near-nuclear powers could build a military nuclear force."

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The drift becomes a rush

Samuel H. Day, Jr.

The government of West Germany is preparing to implement a contract to supply a complete nuclear power cycle, including a fuel reprocessing and uranium-235 enrichment plant, to Brazil. Neighboring Argentina is negotiating with France, as are Pakistan and South Korea, for fuel reprocessing facilities which could be used to recover plutonium for nuclear weapons. The United States and France are competing for a multi-billion-dollar contract which could put Iran in the nuclear weapons production business. And the Soviet Union has become a nuclear reactor supplier for Libya, which has spoken openly of acquiring atomic weapons.

Spurred by the energy crisis and by commercial and military rivalries, the international trade in nuclear technology is rapidly becoming big business. If the present trend continues, many more nations—large and small—will in a short time have achieved full independence in nuclear technology. In the absence of legal and technological restraints, this will also give them the capability of waging nuclear war.

The disarray of the Non-Proliferation Treaty Review Conference last spring put an end to whatever slim

hopes there might have been that governments would soon find a means of stopping the spread of nuclear weaponry.

Rapid progress in the laboratories and on the engineering front makes it futile to hope that whatever barriers remain between commercial capability and military capability can long withstand erosion. There are two continuing reminders of this. One is the recent breakthrough, in the United States and the Soviet Union, in laser beam separation of isotopes, which promises to simplify what has been the most difficult and costly part of the process of producing weapons grade fuel. The other is the steady movement toward a new generation of nuclear power plants (the breeder reactor), which would vastly increase the worldwide inventory of weapons grade plutonium.

The world thus stands literally at the brink of an irreversible proliferation of nuclear warming potential. The fate of millions will then rest with the calculations and miscalculations, the whims and passions of growing numbers of people having the capacity to inflict immediate and almost unimaginable destruction.

This is one element of the problem which makes the atomic threat so much more dangerous today than it was 30 years ago, when the explosion of the first atomic bombs quite rightly provoked a worldwide concern for the future of mankind.

Another element is the continuing unconcern of the older and most formidable nuclear powers with the implications of their nuclear weapons policies. Despite lip service to the concept of strategic arms limitations, it is clear that neither the United States nor the Soviet Union intends to abandon its reliance on ever more sophisticated nuclear weaponry. The Strategic Arms Limitation Talks (SALT) to date have done little more than legitimize and refine the nuclear arms race, rather than halt it. And in the meantime the two governments have moved mountains in their efforts to make nuclear warfare a practical reality. The United States alone has more than 30,000 nuclear warheads stockpiled around the world, perched on delivery systems which are steadily becoming more refined.

In addition to the threat presented by the sheer numbers of such armaments, there is the growing danger of the likelihood of their use. This is implicit in the tendency toward the design and development of tactical nuclear weapons and their deployment (in West Europe and South Korea, for example) in situations where their use might become virtually unavoidable under certain conditions. Step by step the ultimate weapon (in mini-

A note of thanks

James P. Cahill, former executive vice president and general manager of the Educational Products, Inc., Division of Westinghouse Learning Corporation, has joined the *Bulletin of the Atomic Scientists* in the newly created position of publisher. As such, he will have charge of the business operations of the Bulletin under the direction of the editor and the general policy guidance of the Bulletin's Board of Directors.

Mr. Cahill's appointment this summer was made possible by the unprecedented scale of the subscribers' response to the Bulletin's recent financial appeals. We wish to thank all those who helped so generously.

The creation of the position and the appointment of Mr. Cahill mark a significant step forward in the strengthening of the Bulletin as a publishing entity. This step will, we trust, facilitate and reinforce the continued growth of this enterprise, now some 30 years old, as a useful monitor of the critical issues of our times. We hope we may continue to enjoy the support of our readers in that effort.

nuke form, but an atomic bomb nonetheless) becomes incorporated into conventional weaponry. Defense Secretary James R. Schlesinger, backed up by President Ford, has made it clear that the United States does not rule out the first use of such weapons in an emergency.

Given such an example by the world's most developed nuclear power, it is little wonder that other nations are following suit. The Soviet Union attempts to keep pace as best it can with U.S. initiatives in the weapons field. Other governments are now taking steps to redress the nuclear balance and to capitalize on the military and political advantages which come from membership in the nuclear weapons club. The refusal of the two nuclear superpowers to be deflected from their own arms race helped undermine the barrier against the spread of these weapons.

Thus, there is no longer any serious impediment, in law or in technology, to the worldwide scattering of nuclear tinderboxes. The commercial and military momentum is unrelenting. The present national and international safeguards against military adaptation from commercial processes are purely cosmetic. To this must be added the very real and growing danger of diversion not just by governments but by groups capable of fashioning crude nuclear weapons for political or criminal purposes. The long, slow drift toward nuclear catastrophe has now assumed the proportions of a headlong rush.

The third element of the problem is the obliviousness of most people to the seriousness and immediacy of the danger. This is not for lack of repeated warnings. It has become commonplace for heads of state (including those of the United States and the USSR) and other authorities to point out that the problems of nuclear violence remain unresolved. Close observers of the situation (the Stockholm International Peace Research Institute being a notable example) have catalogued the danger in direct detail. And yet the public has remained essentially uncomprehending. Perhaps it is because the Partial Nuclear Test Ban Treaty, which a decade ago transferred most atomic explosions from the atmosphere to underground, removed the most visible signs of the danger. Perhaps it is because of the illusory assurance and comfort provided by SALT and similar ritualistic exercises in the arms control field. Or perhaps it is because, to borrow the apt phrase of Fred Iklé, director of the U.S. Arms Control and Disarmament Agency, the mind simply cannot comprehend, in human and emotive terms, the enormity of the potential toll.

If that is the case, then it is becoming painfully clear

that, sooner or later, a fearful price may be paid for this apathy. Hopefully it will not be paid in the same measure by all of us, although that is by no means beyond the realm of possibility. Surely, though, it would be paid by a very large number, and in a manner entailing the gravest consequences for all mankind. Should this happen, let us hope that the preventive mechanisms sure to follow will also address themselves to other inherently self-destructive qualities in the present international order of sovereign states. And let us hope that the sacrifice will serve not only as a spur to action but also as an undying rebuke to those who failed to act.

Playing politics with U.N. agencies

Stuart A. Rice

Another United Nations specialized agency has taken a long step toward giving first priority to politicization of actions and second priority to the functions it supposedly executes. The World Meteorological Organization at its most recent congress voted to suspend South Africa until its internal racial policies are changed. The same congress voted to invite to WMO meetings observers from several so-called National Liberation Movements.

It is hard to see how such actions can advance cooperation between nations and assist in the provision of needed technological help to those that are in the process of development. The best that can be said is that discriminatory actions by majority groups within organizations that are supposed to be as apolitical as possible reflect an immature but not unexpected use of power by those unaccustomed to the responsibilities which accompany the acquisition of power.

If such power is transitory, to be soon replaced by a return to U.N. principles, perhaps the specialized international agencies can survive. But there is as yet no hint that the politicization of these organizations is temporary—indeed, widely circulated statements by those responsible imply that they are working toward a permanent politicization of all organizations. Unless there is a change in this position soon, the U.N. special agencies seem doomed to fail in the noble aims for which they were created. □

Herbert F. York

The origins of the Lawrence Livermore Laboratory

First there was Los Alamos; then came Livermore, an outgrowth of America's desire to be first with the hydrogen bomb. The rivalry between two U.S. scientific laboratories paved the way for the nation's great leap forward in nuclear weapons design and deployment in the 1950s. The scientist who first directed the Livermore lab recalls the history of that era and finds an important lesson in it.



View of the Livermore weapons lab from the top of its cyclotron building.

The nuclear monopoly of the United States ended when the Soviet Union exploded its first atomic bomb on August 29, 1949. After a sharp, brief and secret debate, President Truman determined that the major response to this particular event should be the development of the *Superbomb*, or H-bomb. This device, estimated then to be 1,000 times as powerful as the first A-bomb, was based on a set of ideas which had been studied on a relatively low priority basis since 1942. In June 1950, North Korea suddenly invaded South Korea, and the United States again found itself at war after less than five years of uncertain peace. In November 1950 great numbers of so-called volunteers from the Chinese People's Liberation Army joined their North Korean comrades and the whole situation took on a more ominous cast.

These historical events as well as three other originally separate strands of developments eventually coalesced and led to the creation of the second U.S. nuclear weapons laboratory at Livermore, California, in the summer of 1952.

One of these strands was the determination of Ernest O. Lawrence and Luis Alvarez to involve themselves and their colleagues at Berkeley in some direct, useful and important way in the U.S. response to the first Soviet A-bomb. Another factor was the protracted conflict between Edward Teller, on the one hand, and Norris Bradbury and his senior staff on the other, about how the Los Alamos Laboratory in New Mexico—the first nuclear weapons laboratory in the United States—might best go ahead on the H-bomb program. This conflict finally led Teller to conclude that another laboratory had to be established to do the job adequately. The third factor, a happenstance, was that a small group of Berkeley scientists, including me, had participated in the George shot, the first thermonuclear test explosion, of Operation Greenhouse, and thus a small cadre of young men at Berkeley were familiar with the details of thermonuclear weapons design.

On the very day the news of the first Soviet A-bomb became known

to the public, Lawrence, Alvarez, Wendell Latimer and others at Berkeley began to ponder the appropriate U.S. response to that event and to search for ways they might participate in such a response. They discussed the matter among themselves, and then traveled to other centers of nuclear research to learn the views of other scientists. Among the places they visited was Los Alamos, where they were particularly interested in learning more about Teller's ideas about the subject.

On the basis of these early explorations, the Berkeley group concluded that they should support Teller's proposals for an urgent, high-priority program at Los Alamos to develop a Superbomb based on the fusion process, and that their group should undertake the design and construction of a reactor which could produce a large excess of neutrons. Teller had explained to them that substantial amounts of tritium—a heavy radioactive form of hydrogen which does not occur naturally—might be needed in the development and manufacture of fusion (hydrogen) bombs, and they knew that the best way to produce tritium was in a reactor specially designed to produce a large excess of neutrons.

The General Advisory Committee (GAC) of the Atomic Energy Commission at its famous meeting in October 1949 decided to recommend against the development of the H-bomb but did agree that the design of such a reactor should be undertaken. The committee also suggested that the program be carried out by the Argonne National Laboratory which had much more relevant experience.

Lawrence and Alvarez were at first disappointed at this turn of events, but they soon responded with an entirely new concept based partly on an idea of Winn Salisbury. It soon became known by its cover name, the Materials Testing Accelerator (MTA), by analogy to the Materials Testing Reactor, which really did have the purpose its name implies. The basic idea of the MTA involved a two step process: first, produce large quantities of free neutrons by brute force and, second,

absorb these neutrons in suitable materials to produce any of several desired end products—tritium, plutonium, uranium-233 or radiological welfare agents.

To accomplish the first step in this process, they proposed building an enormous particle accelerator 60 feet in diameter and more than 1,000 feet long, capable of producing as much as an ampere of deuterons having energies of several hundreds of millions of volts. Such a device would consume some hundreds of megawatts of energy—about what a medium large reactor produces—and it would, they speculated, produce a somewhat larger number of available free neutrons than that same reactor.

Lawrence first asked Robert Serber and me to make separate theoretical estimates of the production of neutrons in such a device, and then asked me to check them experimentally. I had just received my PhD in physics at Berkeley and had stayed on at Lawrence's laboratory, as what today would be called a postdoctoral fellow. My data revealed that suitably large numbers of neutrons would be produced (5 to 10 neutrons per deuteron at 350 million volts) almost no matter what materials were used to construct the primary target for the deuteron beam.

To accomplish the second step in their novel process, they proposed to surround the primary target with a large secondary target lattice in which the free neutrons produced in the first step would be absorbed in a suitable fertile material. In such a system the uranium that had been depleted of uranium-235—that is, the uranium left over after the isotope separation plants and the plutonium production reactors had largely removed or consumed the uranium-235 it originally contained—could be converted into plutonium. In principle the MTA made it possible to exploit the basic raw material, uranium ore, much more completely and efficiently than would otherwise be the case.

Partly because of these potential benefits and partly because of Lawrence's enthusiasm and his reputation as a scientist who "knew how to get things done," the development

and construction of a prototype machine was authorized by Washington. There was, as would be expected, some dispute about such a novel and expensive device, but the Korean war broke out while the project was still in its study phase, and that completely settled the matter for the time being. The Korean war was widely interpreted as a switch by the communists from a program of world conquest by political subversion to a program of direct—if for the moment distant—military action. Since a very large part of our uranium ore came from overseas, mostly the Belgian Congo and South Africa, the worsening world situation was widely perceived as justifying such a Draconian approach to making more complete use of our native supply of ore.

Site Selected

As was customary for such complex nuclear projects, it was decided to proceed by stages. As a first step, a prototype machine suitable for checking out the basic design principles would be built at a site convenient to the Berkeley laboratory. Later, but as soon as possible, a full-scale machine would be built at a site whose selection would be based on other factors, including the availability of sufficient land, operating personnel, electric power, and invulnerability to surprise attack.

A search for a convenient site suitable for the prototype machine resulted in the selection of an abandoned World War II naval air station some 40 miles southeast of Berkeley, at Livermore. The California Research Corporation, a subsidiary of Standard Oil of California, was awarded a contract to build the prototype machine under the scientific guidance of the University of California Radiation Laboratory (UCRL) group, which had by then expanded to include Wolfgang K. H. Panofsky, William Brobeck and many other Berkeley physicists and engineers. It was recognized by all concerned that an industrial contractor would be necessary both to build and to operate a full-scale plant—already planned for construction near St. Louis, Missouri—and the California Research Corporation was brought

in at this stage in order for it to gain the necessary experience as quickly as possible.

The Livermore prototype was actually a full-scale model of the front end of the ultimate machine, and an accelerator 60 feet in diameter and 87 feet long was actually built there. It took longer to build than anticipated, and it never did run well. The MTA turned out to involve technological steps too numerous and too large to be taken successfully all at one time.¹ Before all the bugs were worked out of the design it became clear that the main original reason for building it—the threatened shortage of accessible uranium ore—was no longer valid. Within just a few years after the AEC first anticipated the possibility of a serious shortage, it was discovered that simply raising the price of uranium inspired prospectors to discover very large supplies of ore in Colorado and Canada. As a consequence, the political basis of the MTA was eroded and the Livermore machine was shut down, and dismantling started in December 1953. The MTA building and some of the equipment later were used in support of the Sherwood Program, whose purpose was the controlled production of thermonuclear energy.

In the meantime, however, while I was still refining measurements of the potential neutron yield of the MTA, Luis Alvarez approached Hugh Bradner and me in the spring of 1950 to tell us something about the expanding work on the Superbomb at Los Alamos. He said he had recently talked with Edward Teller about the matter, and it appeared that the project could use some as-

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This article is based in part on a chapter of the author's forthcoming book, The Advisors: Oppenheimer Teller and the Super Bomb.



An abandoned World War II naval air station 40 miles southeast of Berkeley provided the setting for the new facility.

sistance from scientific groups at other laboratories. Bradner and I promptly flew to Los Alamos to meet with Teller and others, and we quickly agreed to set up a special group at Berkeley to perform some diagnostic experiments on the first thermonuclear test explosion, code named the George shot of Operation Greenhouse. About 40 persons were members of this special group, code named the Measurements Project, mostly young physicists who we would now call 'post-docs.' In essence, we were to make experimental observations of certain physical phenomena as these unfolded during the first fraction of a microsecond of the thermonuclear explosion.

I recall that several different considerations strongly motivated and inspired me to participate in the hydrogen bomb program. One was my own perception of the growing seriousness of the Cold War, much influenced by my very close personal student-teacher relationship with Lawrence. The Sino-Soviet Bloc had just been formed; Stalin and Mao both said that it was monolithic and that its goal was world revolution. Another inspiration was the scientific and technological challenge of the experiment itself. It was to be the very first occasion in which a thermonuclear reaction took place on the surface of the Earth, and we were to make complex observations extending over a period of less than a millionth of a second.

Five years before, I had played a peripheral role in the Manhattan Project. I had not participated in the Trinity test of the first A-bomb at Alamogordo, and I had only heard about it a week or so after it occurred. This time I was being invited to participate directly in the heart of the matter.

Another strongly favorable consideration was my discovery that Teller, Hans Bethe, Enrico Fermi, John von Neumann, John Wheeler, George Gamow, and others like them were involved in this project at Los Alamos. They were among the greatest men of contemporary science. They were the legendary yet living heroes of young physicists like myself, and I was greatly attracted by the opportunity of working with

them and coming to know them personally. Moreover, I was not as yet cleared to see GAC documents of deliberations, so I knew nothing about the arguments opposing the Superbomb except for what I learned secondhand from Teller and Lawrence, who, of course, regarded those arguments as wrong and foolish.*

Most of the preparatory work of our group was done in Berkeley. The pilot set-up of our electronics gear, however, required more room than was readily available at Berkeley, so we used some space in the infirmary at the abandoned naval air station at Livermore. The California Research Corporation was already at work on the MTA project at this site, and its working relationship with the University of California Radiation Laboratory made it natural and simple for it to provide us with all the necessary housekeeping functions.

George Shot

During March and April 1951, most of the members of our special group moved out to Eniwetok Atoll in the Marshall Islands and we set up our equipment in its final form in the shadow of the George device. On May 8, 1951, at Eniwetok Atoll the first thermonuclear test explosion on Earth was successfully conducted. The tritium-deuterium mixture burned well and the various diagnostic experiments, including that of our Berkeley group, were successful in recording the various phenomena that accompanied the explosion.

Some of the members of our Berkeley group, after completing the analysis of their data, participated in the general post-experiment discussions and in some of the future planning sessions. No specific plans for further participation resulted from these discussions, however, so the Berkeley group was disbanded. Its members turned to various other projects, mostly pure research in high energy physics.

Shortly before the George shot, Teller and Stanislaw Ulam had contributed the key ideas that resulted in the invention of the basic Super-

*I finally saw the 1949 GAC report for the first time in 1974—a quarter of a century later.

bomb or H-bomb, a device by which a relatively small fission explosion can induce an arbitrarily large thermonuclear explosion. Contrary to much current folklore, these ideas did not directly involve the notion of using as the thermonuclear fuel the easy-to-handle lithium-deuteride salt rather than the very awkward liquid deuterium; that particular idea goes back to a time several years earlier.

Edward Teller stayed on at Los Alamos for only another six months after the George shot at Eniwetok. The next major experiment, the Mike shot of Operation Ivy,* was to be based on the Teller-Ulam ideas, and Teller participated directly in the determination of its basic configuration. But in November 1951, a year before Mike was to be fired, he left Los Alamos and returned to the University of Chicago. He did so because he felt that the remaining theoretical work needed to get Mike ready could be done just as well without him—Bethe was already scheduled to be at Los Alamos during the final design period—but mainly because of the long standing and increasingly acrimonious argument between him and Bradbury over how to run the laboratory and the hydrogen bomb program.

An Open Secret

In the summer of 1951, only some months after he had come up with the final, capping suggestion in the series of ideas that led to the invention of the Superbomb, Teller had concluded that the establishment of a second, independent laboratory was needed to exploit this new approach in a timely fashion. As Teller later put it:

It was an open secret, among scientists and government officials, that I did not agree with Norris Bradbury's administration of the thermonuclear program at Los Alamos. Bradbury and I remained friends, but we differed sharply on the most effective ways to produce a hydrogen bomb at the earliest possible date.

*The first thermonuclear reaction was achieved with the Greenhouse-George test but the Ivy-Mike test on November 1, 1952, achieved the first huge thermonuclear explosion: the detonation measured about 10 megatons and erased the island of Elugelab from the map.

We even disagreed on the earliest possible date itself, on the timing of our first hydrogen bomb test. The dissension with Bradbury crystallized in my mind the urgent need for more than one nuclear weapons laboratory.²

Teller soon succeeded in persuading Gordon Dean, then Chairman of the AEC, to consider the question of establishing a second laboratory. Dean in turn asked the General Advisory Committee of the AEC to review the idea. Except for Willard Libby, like Teller a professor at the University of Chicago and then a new member of the committee, the GAC opposed the idea on the ground that the establishment of a second laboratory would divert talent and resources from Los Alamos and would thus slow down the overall program. Personal elements were probably also importantly involved in the opposition of the GAC to a second laboratory.

Teller's claim that competition was a good thing was often expressed in terms which made it clear he felt the Los Alamos leadership was unimaginative, negative, and otherwise inadequate. It was equally clear that Teller felt much the same way about many of the members of the GAC itself, and so it is not surprising that the GAC supported Los Alamos and Bradbury against what they regarded as an unwarranted personal attack.

Teller also sought support of his ideas in the Air Force. It seemed that they would be the principal user of the hydrogen bomb, and a number of persons at the top of the Air Force very quickly showed great personal and institutional interest in the issues being raised. David Griggs, one of the founders of the Rand Corporation and just then Chief Scientist of the Air Force; James H. Doolittle, a much respected retired General and a high-level consultant to the Air Force; and General Elwood R. Quesada, the commander of the joint task force that had conducted Operation Greenhouse, all became strong partisans of Teller and helped him make further contact with higher Air Force officials.

As a result, Teller and his ideas were warmly received and strongly endorsed by Thomas K. Finletter,



Preparations for an explosion at the Nevada Test Site. The nuclear detonation point is hundreds of feet below the place at left where instrument cables converge.

Secretary of the Air Force, and his special assistant for R&D, William A. M. Burden.* They, in turn, began to make moves toward establishing a second nuclear weapons laboratory under Air Force sponsorship. In 1951 they actually did arrange to sponsor briefly some nuclear calculations Teller was doing at Chicago. However serious their intentions concerning a full-scale second laboratory may have been, their actions greatly increased the pressure on the AEC either to do something itself or to see its monopoly in the field eliminated.

During this period, Teller also was given the opportunity to brief Secretary of State Dean Acheson, Secretary of Defense Robert Lovett, and Deputy Secretary of Defense William C. Foster. The very fact of these briefings, independent of their specific content or results, put further pressure on the AEC.

In late 1951, Thomas Murray, the Atomic Energy Commissioner most sympathetic to the idea of a second laboratory, got in touch with Ernest Lawrence to discuss the matter with him. Lawrence responded positively, and volunteered to study the question further.

Since I had been more deeply involved in the recent thermonuclear program than anyone else at Berkeley, Lawrence in January 1952 asked me what I thought. As a direct result of Lawrence's inquiry, I made a series of extended trips to Los Alamos, Chicago, and Washington, where I discussed the matter with Teller and most of the people named above plus a few others, including Army General Kenneth Fields, then the AEC's Director of Military Applications, and his deputy, Navy Captain John T. Hayward. I found the whole affair heady and exciting, and I was readily persuaded to Teller's point of view. I reported to Lawrence that I, too, felt it would probably be useful to establish a second laboratory..

The idea of doing so at the Livermore site was, for us, a natural one,

*Not to be confused with William L. Borden, then Executive Director of the staff of the Joint Committee on Atomic Energy, and the man who later first formally charged J. Robert Oppenheimer with being a Soviet agent.

and we suggested it immediately to the AEC. This proposal to establish facilities at Livermore as a branch of the University of California Radiation Laboratory instead of simply establishing one somewhere under an unspecified aegis, changed the nature of the argument. It clearly meant much less initial expense and an immediate, if small, cadre of people ready to go to work right away. As a result, as GAC Chairman Oppenheimer later recalled, the GAC and the AEC "approved the second laboratory as now conceived because there was an existing installation, and it could be done gradually and without harm to Los Alamos."³



Edward Teller,
prime mover of the new lab.

As I recall it, Lawrence and Teller felt at the time that Oppenheimer himself was still really opposed to a second laboratory but that under the new circumstances he had no other choice. Even so, during that year I met with Oppenheimer at Princeton and discussed the plans for the Livermore laboratory. He received me in a personally friendly fashion, but I cannot recall his being of any particular help.

The precise nature of the plans for the new laboratory, however, primarily reflected Lawrence's ideas about how to go about such things and deviated considerably from Teller's views of what should be done. In essence, Lawrence firmly believed that if a group of bright young men were simply sent off in the right direction with a reasonable level of support, they would end up in the right place. He did not believe

that the goals needed to be spelled out in great detail or that it was necessary that the leadership consist of persons who were already well known. Teller, on the other hand, had become deeply suspicious of the intentions of the AEC leadership, and he wanted something more nearly analogous to the 1943 plans for Los Alamos—a plan for a laboratory with a well-defined goal that would be led by a large cadre of famous scientists.

To complicate matters, during that spring Lawrence, suffering from a chronic illness, spent much time away from Berkeley on long rest trips. As a result I was left pretty much on my own to draw up the specific plans for a second laboratory with nothing except the most general guidance from my immediate superior. However, as a result of 10 years close association, I both clearly understood and firmly agreed with Lawrence's approach to 'big science,' and I generated plans which he always warmly endorsed when he had a chance to review them.

Finally, and in close accordance with Lawrence's (and my) views of the matter, the AEC in June 1952 approved the establishment of a branch of the Berkeley laboratory at Livermore to assist in the thermonuclear weapons program by conducting diagnostic experiments during weapons tests and other related research. The question of how soon, or even whether, the Livermore Laboratory would actually engage directly in weapons development was left open, however. The AEC's official planning document described the mission of the Livermore Laboratory this way:

a. Development and experimentation in methods and equipment for securing diagnostic information on behavior of thermonuclear devices and the conduct of such instrumentation programs in support of tests of thermonuclear devices, in close collaboration with the Los Alamos Scientific Laboratory.

b. While the work authorized above is the immediate objective of this proposal, the Commission hopes that the group at UCRL (Livermore) will eventually suggest broader programs of thermonuclear research to be carried out by UCRL or elsewhere. (Emphasis added).⁴

Lawrence felt that this statement of intentions provided an adequate base upon which to build a second weapons laboratory. I would have preferred something more concrete, but I was prepared to accept it as a place to start from. Teller, on the other hand, found the vagueness of the AEC's plans for the Livermore Laboratory entirely unsatisfactory.

As a result, in early July he told Lawrence, Gordon Dean, myself and others that he would have nothing further to do with the plans for establishing a laboratory at Livermore. The Berkeley administration was prepared to go ahead anyway. However, at the insistence of Captain John T. Hayward (then Deputy Director of the AEC's Division of Military Applications), more than anyone else, intense negotiations were resumed among all concerned. Within days this led to a firm commitment on the part of Gordon Dean that thermonuclear weapons development would be included in the Livermore program from the outset, and also to a renewed commitment on the part of Teller to join the laboratory.

The Livermore Laboratory was launched in September 1952. I became the director, and the Scientific Steering Committee included Teller, Harold Brown, John S. Foster, Jr., Arthur T. Biehl, and a few others. Teller, because of his obvious special status, was given veto authority over the decisions of the committee but otherwise had no formal authority. Brown was put in charge of the development of thermonuclear weapons at Livermore; Biehl at first and then Foster was put in charge of the development of improved fission weapons. There were some early problems in the administration of the theoretical division; but these were resolved, on a temporary basis, by appointing Richard Latter as acting head of the division. Latter, a Rand physicist, was temporarily on loan four days a week to the Livermore Laboratory. After about a year, he was replaced by Mark M. Mills, who remained in that position until his death in a helicopter crash at Eniwetok in early 1958. These organizational arrangements, although they contained some peculiar ele-

ments, worked out very well and none of the strained relationships that had surrounded Teller at Los Alamos developed at Livermore.

L'Affaire Mike

The relationship between the Livermore and Los Alamos laboratories was strained from the beginning, and rapidly grew worse. One of the causes of this state of affairs involved the question of who deserved the credit for Mike, the first large thermonuclear device.

In November 1952, two months after the Livermore Laboratory was established, Mike was exploded during Operation Ivy. Based on ideas by Teller, Ulam, and many



Norris Bradbury,
guardian of the old one.

others, it was designed, built and tested by the Los Alamos Laboratory. Los Alamos was aided in those tasks by a number of other institutions, including the Naval Research Laboratory and the Matterhorn-B group at Princeton; but not by the Livermore Laboratory, which then was completely absorbed in simply coming into being. Nevertheless, the press commonly gave Teller, and by implication the Livermore Laboratory, the whole credit.

The reasons for this error were Teller's presence at Livermore and the absurdly strict secrecy policy on the part of the AEC. The AEC, purportedly for security reasons, refused either to make or to allow any comment whatsoever on the origin and nature of Mike, not even the simple denial of its being created by Livermore. This situation continued for almost two years. It was not until

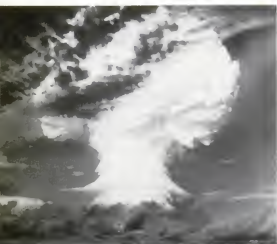
after Shepley and Blair brought out a biased account of the matter in 1954 and excerpts were published in *Life* magazine that Bradbury was finally allowed to respond by holding a press conference giving his version of history.⁵

Relations between Livermore and Los Alamos, already bad, became worse during the hearings on J. Robert Oppenheimer's security clearance. Oppenheimer's clearance had been lifted in late 1953 and hearings in the matter held in the spring of 1954. Despite elaborate efforts to assert the contrary, the real stimulus for the removal of Oppenheimer's clearance was his opposition to the program for the development of the H-bomb.

Teller, along with Lawrence, Alvarez, Wendell Latimer, and some others at the University of California provided some of the most important arguments in support of the government's case. On the other hand, both the veterans of the wartime Los Alamos Laboratory as well as its leadership during that time strongly supported Oppenheimer. All during those very trying times, the Los Alamos administration treated the Livermore leadership with formal correctness and provided some much needed technological assistance to the new laboratory. But some leading scientists at Los Alamos made it clear that Teller was personally unwelcome there. Since Teller was such a central figure at Livermore, this feeling inevitably affected the relationship between the laboratories.

On top of all that, the first Livermore tests—in Nevada in 1953 and at Bikini in 1954—went poorly. Given the attacks on the quality of the leadership at Los Alamos that were part of the arguments supporting the establishment of a second laboratory, it is not surprising that some Los Alamos scientists filled the air with horse laughs on those occasions.

The Livermore Laboratory flourished nonetheless and eventually produced its share of new weapons ideas and designs. From its first year of operations (fiscal 1953) with a budget of \$3.5 million and a staff of 698 the laboratory grew steadily.



The Mike shot, first of the large thermonuclear explosions, at Eniwetok in 1952. After the blast came a furor over the credit.

Five years later (fiscal year 1958) during my last year as director, the staff passed the 3,000 mark and the budget reached \$55 million. In another five years the staff passed 5,000 and the budget reached \$127 million. Shortly after that, the laboratory stabilized at a population of about 5,500.

The establishment of the second nuclear weapons laboratory at Livermore—renamed the Lawrence Livermore Laboratory after Lawrence's death in 1958—in effect doubled the number of scientists, engineers and technicians working on nuclear weapons. But this doubling of the number of persons involved did much more than simply increase the rate of progress by a factor of two. As Teller said in explaining why he left Los Alamos and undertook the promotion of a second laboratory, technology, like so many other elements of human endeavor, does thrive on competition. I have little doubt that the Livermore Laboratory was seen as competition by Los Alamos, and I know for a fact that we at Livermore saw ourselves as being in competition with them.

Within 10 years the new burst of intellectual energy, which both laboratories focused on nuclear weapons design problems, resulted not only in a great profusion of relatively large hydrogen bombs for ICBMs and the like, but also in varieties of nuclear weapons that weighed less than 100 pounds (as the Davy Crockett), that were only inches in diameter (as for artillery shells), and that were on the average sufficiently efficient in their use of nuclear materials so that they could number many tens of thousands.

A Great Leap Forward?

The introduction of such a large variety of weapons led directly to their being stored in a great many different locations abroad as well as at home, and to a correspondingly great proliferation in the number of military command elements having immediate control of the individual weapons. There can be little doubt that the great leap forward in nuclear weapons design and deployment that occurred in the 1950s resulted in part from the creation of the Law-

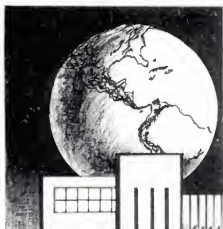
rence Livermore Laboratory. Whether this accelerated rate of progress has had a beneficial or harmful effect on national security is another interesting and important question. I believe though the net effect has been harmful; but the reasons why I think this is so are beyond the scope of this article.⁶

My purpose in recounting the history of the Livermore Laboratory is twofold. One purpose is to help today's citizens understand a little better how the world they see about them came to be. Another purpose is to help provide a piece of the factual background needed for the current restudy of America's national goals and needs.

A third of a century ago, the terrible events that made up the early stages of World War II brought about the creation of the U.S. nuclear weapons development program. A quarter century ago, the very threatening events that marked the darkest years of the Cold War stimulated a large increase in the pace of the U.S. nuclear program, including but not limited to the establishment of the second nuclear weapons laboratory. Now, with our longest war ended and peace reestablished, it is necessary and appropriate to undertake once again the same kind of questioning and replanning of the nuclear activities of the United States that took place in 1949-1952. Surely reasons or rationalizations a third and a quarter of a century old cannot be automatically accepted as the basis for current policies. Contemporary programs must be based on contemporary reasons.

Notes

1. The only published account of the MTA project is a "gee whiz" article by Allen P. Armaguer, "The Most Fantastic Atom-Smasher," *Popular Science*, Nov. 1958, pp. 108ff.
2. Edward Teller with Allen Brown, *The Legacy of Hiroshima* (Garden City, N.Y.: Doubleday & Co., 1962), pp. 54-55.
3. In the matter of J. Robert Oppenheimer (Cambridge, Mass.: MIT Press, 1971), p. 248.
4. Atomic Energy Commission, Director of Military Applications, *Thermonuclear Research at the University of California Radiation Laboratory*, AEC 425/20 (Washington, D.C.: The Commission, June 13, 1952).
5. Shepley and Blair, *The Hydrogen Bomb*, New York, 1954. Teller's version is given in *The Legacy of Hiroshima*, and in *Science*, Feb. 25, 1955, p. 268.
6. Herbert York, *Race to Oblivion: A Participant's View of the Arms Race* (New York: Simon & Schuster, 1970), chap. 2, pp. 27-48; York, "We Can Reverse the Arms Race: A 10-Point Plan," *Life*, 69(Dec. 11, 1970), 40-41; York, "Controlling Qualitative Arms Race," *Bulletin*, 29(March 1973), 4-8; York, "Reducing the Overkill," *Survival*, 16(March/April 1974), 70-74; and York and Jerome Wiesner, "National Security and Nuclear Test Ban," in *Arms Control* (San Francisco: W. H. Freeman & Co., 1973), pp. 129-138.



Editor's foreword

As readers of the *Bulletin* well know, few issues have been more vigorously disputed in recent years than the question of nuclear reactor safety—and, in particular, the risks and consequences of major accidents. We ourselves have been skeptical of industry and government assurances in this area. We continue to be. The following special report, capitalizing on the most recent studies of reactor safety, reflects a variety of opinions, although by no means the full spectrum, on the subject. We offer it not as the last word but as a current analysis, and in accordance with our tradition of keeping the *Bulletin* open to differing points of view.

—S. H. D.

Joel Primack

Two documents have recently had a significant impact on the continuing controversy over the safety of nuclear reactors: The U.S. Atomic Energy Commission's Reactor Safety Study¹ and the "Report to the American Physical Society by the Study Group on Light Water Reactor Safety."² This special report of the *Bulletin* is an attempt to review the reactor safety controversy in the light of these reports—to identify areas of agreement and disagreement, and to explore some of the implications for national policy. The authors include Norman C. Rasmussen, who directed the AEC's study; Fred C. Finlayson and Frank von Hippel, two of the authors of the APS study; Her-

NUCLEAR REACTOR SAFETY

An introduction to the issues

Substantial questions have been raised; only expanded research and development efforts can provide answers beyond serious dispute

bert J.C. Kouts, the Director of the Office of Nuclear Regulatory Research at the Nuclear Regulatory Commission,* and Robert K. Weatherwax, a specialist in probabilistic assessment of risk. These papers were originally presented at a symposium held on April 30, 1975, at the annual spring meeting of the American Physical Society in Washington, D.C. Hans Bethe, who chaired the symposium, has also contributed a short paper for this special report.

A typical modern power reactor, which produces 3,300 megawatts of thermal energy (MWt) and 1,000 megawatts of electrical energy (MWe),** contains about a billion curies of radioactive fission fragments in its core. Their radioactive decay heat accounts for about 7 percent of the reactor's power output in normal operation—the remainder coming from the energy

released in the nuclear fission itself. Unlike fission, however, radioactive decay continues to generate heat even after the reactor has been switched off. Uncooled, this decay heat could rapidly melt the reactor core, melt through the thick steel of the reactor's pressure vessel, through its concrete floor, and on down into the earth, possibly releasing very large quantities of radioactive gases to the atmosphere.

In order to prevent such a 'meltdown' accident, all large commercial reactors in the United States are equipped with emergency core cooling systems (ECCS), which are supposed to arrest meltdown in the event of an abrupt loss of primary coolant water. Unfortunately, these emergency cooling systems were designed on the basis of highly simplified computer programs; and scale-model tests and other experimental results available in early 1971 turned out much worse than had been expected. As a result, the Union of Concerned Scientists and other groups issued public critiques and intervened in reactor licensing hearings;³ the AEC in response con-

*The Nuclear Regulatory Commission assumed the regulatory responsibilities originally vested in the Atomic Energy Commission when the Commission was split into two agencies in January 1975, namely, the NRC and the Energy Research and Development Administration (ERDA).

**A reactor rated at 3,300 MWt produces 3.3 billion watts of thermal energy (the total heat output); of this one billion watts is converted to electricity (1,000 MWe) and the remainder (2,300 MWt) is waste heat.

Joel Primack, assistant professor of physics at the University of California at Santa Cruz, is the guest editor of this special report of the *Bulletin*. He initially proposed the American Physical Society's energy studies and also organized the APS symposium at which the papers in this special report were first presented.

vened a major national hearing on emergency core cooling systems. These events are reviewed in Finlayson's article. The ECCS hearing and the subsequent AEC Opinion did not entirely succeed in resolving the emergency core cooling issue, as Finlayson explains.

The ECCS debate is the most recent chapter in a controversy which has been going on now for some time.⁴ In recent years, both the nuclear critics and the AEC itself have focused much of their attention on the consequences of the worst possible reactor accidents. The critics have long cited WASH-740, a 1957 AEC report which concluded that a major release of radioactivity from even a small reactor could kill thousands of people, injure tens of thousands more, and cause billions of dollars in property damage.⁵ Of course, no one expected such serious accidents to occur very frequently; an estimate common in the nuclear community was that a meltdown accident might occur perhaps once every million reactor years. But the rather high frequency of smaller accidents and the unresolved ECCS controversy left some observers with a feeling of uneasiness.

In August 1972 the AEC commissioned a new study of reactor safety, to be performed by a team of AEC employees and outside consultants under the direction of Norman Rasmussen. The Rasmussen group was instructed to calculate not only the consequences but also the probabilities of a broad spectrum of possible reactor accidents. By thus focusing on the average risk, the AEC hoped to obtain a report both more realistic and less alarming to the public than the worst-case assessment of WASH-740 and its even more alarming but unpublished sequel, prepared in 1964.⁶

Two Major Surprises

As expected, the AEC's draft WASH-1400 report concluded that the average risk to the population from nuclear reactor accidents was indeed very low: for example, over a period of many years, only about 6 persons per year were expected to be injured, and 0.3 persons per year killed, by a reactor accident at any

of the first 100 reactors in the United States. However, the Rasmussen group's calculations contained two major surprises: (1) that meltdown accidents were not expected to be extremely rare events after all, and (2) that most meltdown accidents were unlikely to cause much damage *outside* the nuclear installation itself.

Most previous examinations of meltdown accidents, including those which are part of the NRC's licensing criteria for reactor emergency core cooling systems, have concentrated on analyzing a 'design basis accident' consisting of a 'double-ended guillotine break' in one of the main primary-cooling-system pipes. But the Rasmussen study showed that meltdown accidents can also be initiated by breaks in small pipes or by other events (reactor 'transients') which are considerably more likely to occur than large pipe-breaks; and that in some of these circumstances the ECCS can actually exacerbate the accident. Taking into account the spectrum of possible initiating events, the Rasmussen group calculated that the probability of a meltdown accident is roughly 10^{-4} (or one chance in 10,000) per reactor year. With the 100 reactors expected to be operating in the United States by 1980, this implies a one percent chance of such an accident each year.

That was the bad news. But the good news—again contrary to popular belief, and this time contrary also to several previous AEC studies—was that most meltdowns would be essentially innocuous. Nine times out of ten almost all of the dangerous radioactivity would just melt quietly down into the ground. Even if a radioactive cloud should escape from the reactor containment building, it would disperse almost harmlessly in the vast majority of weather conditions, according to the Rasmussen report. Thus, this draft report estimated that the chance of a meltdown followed by a major release of radioactivity (the 'reference accident' in the Rasmussen study, which was analyzed also in the APS study) is about 10^{-8} (one in 100,000) per reactor-year. The chance of a really catastrophic acci-

dent was estimated to be only 10^{-9} (one in a billion) per reactor-year, as Rasmussen explains in his article in this issue.

Conclusions Attacked

In systematically applying equipment failure-rate data and probabilistic methods to the analysis of a very large range of possible reactor accidents, the Rasmussen group has broken important new ground. Their techniques will certainly find continuing applications in pointing the way toward increased reactor safety. Without disputing the value of much of the work reported in their draft report, however, a number of critics have attacked two of the report's essential conclusions: first, that the average risk can actually be reliably estimated within the quoted errors of a factor of 3 either way; and, second, that the consequences of even a large release of radioactivity in a reactor accident are likely to be rather minor.

Weatherwax discusses a number of ways in which the risk calculations presented in the Rasmussen report can be improved within the framework of the present methodology. He also identifies several significant deficiencies in the methodology itself; for example, human errors are important, but very hard to quantify. (Here, it should be emphasized that both Rasmussen and Weatherwax only considered bona fide human *errors*, not deliberate sabotage.) The various reactor systems also may fail, not only because of equipment or operator failures, but because the engineering designs themselves are inadequate (as the emergency core cooling system may be in certain circumstances). For such reasons, Weatherwax concludes that it is inappropriate to compare the risk estimates stated in the AEC's draft report with actual statistical data for other types of accidents. Rasmussen, in his article, explains why he disagrees.

The articles by von Hippel and Bethe discuss the APS study group's criticisms of the reactor accident consequence calculations made by the AEC's study group. The main corrections concern the number of persons expected to die from lung

reactor safety glossary

design features

Nuclear reactor: a device in which a fission chain reaction can be initiated, maintained, and controlled. A *light water reactor* (LWR) is one which is cooled and moderated by ordinary water. The function of the moderator is to slow down fast neutrons that are produced in fission, since slow neutrons are much more likely to cause fission in a uranium-235 nucleus. The two basic types of LWRs in commercial use in the United States today are the pressurized water reactors (PWRs) and the boiling water reactors (BWRs). Both types are fueled with uranium that has been enriched to contain 2 to 4 percent uranium-235. The fuel is sealed in tubular metal (zircaloy) cladding, the fuel rods.

BWR: *boiling water reactor;* a LWR in which the water is allowed to boil in the pressure vessel. The steam generated in the pressure vessel passes directly to the turbine generators, the 'direct cycle' system.

As of June 30, 1975, there were 23 commercial BWRs in operation or licensed by the NRC in the United States, and 53 BWRs under construction, on order, or planned.

PWR: *pressurized water reactor;* a LWR in which the water is not allowed to boil anywhere in the pressure vessel. It is kept under high pressure and high temperature to keep it liquid. The heat is transferred to a separate stream of water which boils and produces steam for the turbine. All PWRs employ this dual system for transferring energy from the reactor fuel to the turbine and are called 'indirect cycle' systems.

As of June 30, 1975, there were 30 commercial PWRs in operation or licensed by the NRC in the United States, and 121 PWRs under construction, on order, or planned.

Pressure vessel: the largest and heaviest piece of equipment in a nuclear power plant which houses the fuel core, mechanisms for driving control rods into and out of the core, and pipes for circulating the cooling water.

Downcomer: cylindrical passage between the reactor core and pressure vessel through which the

cooling water circulates downward from inlet nozzles before passing upward through the core.

safety mechanisms

Scram: emergency shutdown system for rapidly terminating the reactor chain reaction. When a planned or unplanned event or accident occurs wherein the reactor has to be shutdown, the performance of a number of systems is involved; their failure could lead to serious problems, including loss of cooling.

ECCS: *emergency core cooling system(s);* the fission reaction that produces most of the heat in a nuclear reactor can be turned off at any time by inserting control rods, containing strong neutron-absorbing materials, into the reactor core. However, the heat produced by radioactive decay cannot be turned off. Thus, a system must be provided to cool the core and to prevent the fuel rods from melting, with potentially catastrophic results, if normal cooling is interrupted. This is the emergency core cooling system, which to date has not been given a full-scale test on a working reactor.

A major forthcoming program at the Idaho National Engineering Laboratory, the loss of fluid test (LOFT), will examine what happens during loss of coolant accidents (LOCA), and how the ECCS behaves in such circumstances at the system test level. The LOFT test reactor is a small PWR designed to carry out LOCA experiments on a size and scale (55 MWt) between that of a laboratory experiment and that typical of a commercial PWR (3,300 MWt).

accidents

Transients: events, planned or unplanned, which sometimes lead to a signal to terminate the chain reaction (shut down the reactor). The reactor control system can automatically adjust for some transients without service interruption.

LOCA: *loss of coolant accident.* An accident of this sort may range in severity from a catastrophic rupture of the reactor pressure vessel (which is almost certain to lead to a core melt with large potential

radiological consequences to the public) to rather minor leaks in pipes (which could be handled by the engineered safety features of the reactor).

Blowdown: first phase of a LOCA. The water in the reactor cooling systems is at a very high pressure and if a rupture occurs in the pipes, pumps, valves or vessels that contain it, the rapid pressure release would result in some of the water flashing to steam. The resulting pressure will force most of this water-steam mixture out the broken pipe.

Meltdown: if a failure in the reactor's cooling system occurs that allows the fuel to heat up to its melting point (about 5,000°F), a core meltdown would occur.

If the core melts, existing reactor safety systems would be unable to cool it and a core melt-through would follow. The molten core would melt through the pressure vessel, containment building, and sink into the earth below (the 'China syndrome').

Containment failure: a major release of gaseous and volatile fission products to the atmosphere as a result of the breach of the containment building—the large, dome-like structure surrounding the reactor—following a meltdown.

Reference accident: a core meltdown for a 1,000 MWe pressurized water reactor with a subsequent containment failure. This accident, used to set a scale for reactor accident consequences, is the accident for which the most complete information is given in WASH-1400 Draft, and is the accident which the APS study used to compare their analysis with the AEC's study.

Health effects: a term used in these articles to denote the consequences to human health of a meltdown followed by a containment failure.

Early, immediate or short-term 'health effects' are fatalities occurring in the first one to two months as a result of acute radiation.

Delayed or long-term 'health effects' are fatalities (cancer deaths) occurring over a period of several decades. Other 'health effects' include genetic defects and thyroid nodules.

and thyroid cancers, and from other cancers induced by the long-term low-level exposure of the potentially large population living up to distances of 500 miles or so downwind of a major release of radioactivity. The AEC's report estimated about 300 cancer deaths for its 'reference accident'; the APS estimate is 10,000. Bethe notes, however, that the AEC probability estimate for the 'reference accident' (about 10^{-5} , one in 100,000, per reactor-year), combined with the APS corrected casualty estimate, still gives a very low average risk per year. Nevertheless, the APS corrections reemphasize the seriousness of any large-scale release of radioactivity. Furthermore, as Finlayson points out, the public aversion to major catastrophes is probably not easy to understand in terms of an annualized risk estimate.

Most of the U.S. light water reactor safety research is now conducted under the sponsorship of the Nuclear Regulatory Commission. The keystone of this program since the early 1960s has been the Loss of Fluid Test Facility (LOFT), located at ERDA's Idaho National Engineering

remain the very serious problem of scaling up from the small LOFT reactor to modern commercial power reactors, which are typically some 60 times larger. The elaborate computer programs now being developed to predict the behavior of large power reactor ECCS systems are full of adjustable parameters. Even if these computer programs succeed in modeling the behavior of the LOFT reactor in simulated accidents—which is by no means certain—it will still be unclear what values of the adjustable parameters would be appropriate for full scale reactors.

The APS study reaches the sobering conclusion that the presently planned research program gives no assurance of actually being able to resolve the serious questions which have been raised about reactor safety. Therefore, they recommend that the scope of the reactor safety program be substantially broadened and the funding level increased, so that several promising approaches can be pursued which are presently being neglected. They are especially concerned that alternative emergency cooling system designs be developed and tested, so that we are not

being devoted to these tasks out of a total light water reactor safety research budget of roughly \$70 million (fiscal year 1975); almost all the money is presently going to studies of ECCS performance.

The recommendations of the APS study are discussed in more detail in the articles by Finlayson and von Hippel; Kouts, in his article, discusses a number of areas of reactor safety research that the NRC is considering for expansion in light of the APS study.

Behind this discussion of nuclear energy is the tremendous pressure to build reactors in order that we may continue to double our electricity consumption each decade. As of June 1975 there were 55 nuclear power plants operating in the United States. Roughly 50 more reactors will be completed in the next five years, some 300 are sought for the decade of the 1980s, another 600 for the decade of the 1990s, and so on. Since the construction of large reactor installations typically takes nearly a decade, to stay on this ambitious schedule would require that work be started on several hundred new reactors in the next few years. It is extremely unfortunate that the basic safety research, which would have led either to full confirmation of the adequacy of existing reactor emergency systems or to design of better ones, was not completed before now—but the fact remains that much of this research remains to be done. Of course, we can just go ahead and build large numbers of reactors of present design and be prepared to live with the consequences, which will in any case probably be of far smaller total social cost than an equivalent number of coal-fired power plants of present design. But it would clearly be preferable to be able to build reactors whose safety is beyond serious dispute.

The APS study holds out considerable hope that this goal can actually be achieved, but only if a greatly expanded research and development effort is undertaken. The total currently spent on reactor safety by the federal government, reactor manufacturers and utilities is less than \$100 million annually. This

Since the United States is planning to rely upon nuclear energy for many decades, it would seem both wise and fiscally prudent to embark immediately upon a truly ambitious reactor-safety program.

Laboratory near Idaho Falls. The APS study group devoted much of its efforts to evaluating LOFT and other components of the NRC's reactor safety program. They were favorably impressed by the NRC's laboratory studies of the separate effects bearing on ECCS performance—heat transfer, two-phase (steam-water) flow, etc. But they emphasize in their study that important results of the separate effects tests and the LOFT program are years away, "perhaps five to eight [years] at the presently planned level of program resources."

Furthermore, even after this information is in hand, there will still

be forced to continue to rely exclusively upon the present somewhat uncertain ECCS concepts. A new, more convincing ECCS design—one which would be relatively easy to analyze and implement—could result in greater confidence in the margin of safety, faster licensing of reactors, and other clearly desirable benefits.

As a backup program, the APS study recommends that much greater effort be devoted to improving reactor containment, mitigating the consequences of a large release of radioactivity, and learning how to clean up afterward—just in case. At present, only about \$1 million is

amount is nearly a full order of magnitude less than the cost of one large reactor installation; it is only about 3.5 percent of the present annual cost of electricity generated by U.S. commercial reactors, and a much smaller fraction of the projected revenues from reactor generated power in the future. Since the United States is planning to rely upon nuclear energy for many decades, it would seem both wise and fiscally prudent to embark immediately upon a truly ambitious reactor-safety program.

Two other issues in addition to reactor safety frequently arise in discussions of nuclear energy: the problem of disposing of high-level radioactive waste, and the danger of nuclear weapons proliferation and terrorism. Without in any way denigrating the importance of these issues, I nevertheless believe that resolution of the reactor safety problem—to which this special *Bulletin* issue is devoted—is of special urgency now, as the nation moves toward a massive reactor construction program. The point is that the waste-disposal problem can be postponed: high-level waste can be solidified and stored in remote locations until some final disposal technique is agreed upon. On the other hand, nuclear proliferation and terrorism are essentially international problems, since European nations as well as other nations seem determined to build nuclear technologies of their own—and some have far fewer alternative sources of energy than we do. Unfortunately, it is evidently not very hard to make bombs; and it is presumably no more difficult to move nuclear materials or weapons across national borders than it is to transport an equivalent quantity of marijuana. I am personally very much worried about proliferation and terrorism, and I believe that the U.S. State Department and other government agencies should devote increased attention to this problem regardless of how many reactors we decide to build in the United States.

And how many reactors *should* the United States plan to build? There are some environmentalists



"Our findings are entitled 'Periodic Escalation to Prepare and Plot Equal Retaliation.' PEPPER."

and others who seek to call a complete halt to reactor construction, largely, it seems, as a vote of no confidence in the institutions which have been managing the nuclear program to date. While it is true that the record of the AEC, the utilities, and the reactor manufacturers has not been particularly inspiring, it is not clear that terminating the U.S. nuclear program is the most appropriate response. On the other extreme, there are some—including Presidents Nixon and Ford—who want to build reactors with maximum feasible haste: 200 reactors by 1985 and 1,000 reactors by the end of the century. Those who favor such a crash program usually extrapolate past growth in U.S. energy consumption into the future and point with trepidation to the greatly

increasing U.S. dependence on foreign oil. The work of the Ford Foundation's Energy Policy Project⁷ and others, however, indicates that we can greatly increase the flexibility of our energy policy if we will systematically adopt energy-conserving technologies in all sectors of the economy. The Ford study projects a requirement for only a few hundred nuclear reactors in the year 2000, assuming what they consider to be a reasonable amount of energy conservation (their "Technical Fix" scenario) and moderate development of U.S. coal resources. Thus we can perhaps afford to build reactors at a relatively relaxed pace. This would be fortunate, since a crash program—as von Hippel points out—may be incompatible with significant improvements in reactor safety.

Notes

1. Atomic Energy Commission, "Reactor Safety Study—An Assessment of Accident Risks in U.S. Commercial Power Plants," WASH-1400 Draft (Washington, D.C.: The Commission, Aug. 1974).

2. This two-year-long, \$1 million study was sponsored by the AEC and performed under the direction of Norman C. Rasmussen of the Massachusetts Institute of Technology. We refer hereinafter to this study, and the draft report, as WASH-1400 or the Rasmussen report.

3. American Physical Society, "Report to the APS by the Study Group on Light Water Reactor Safety," presented at 1975 spring meeting of the Society. See *Reviews of Modern Physics*, vol. 44, supplement 1, 1975. We refer hereinafter to this study as the APS study.

4. This year-long study was sponsored by the APS and jointly financed by the former Atomic Energy Commission and the National Science Foundation. The study group was chaired by Harold W. Lewis, professor of physics at the University of California, Santa Barbara. Other members of the group were: Robert J. Budnitz, Lawrence Berkeley Laboratory; Albert W. Castleman, Jr., Brookhaven National Laboratory; David E. Doran, University of California, Santa Cruz; Fred C. Finlayson, Aerospace Corp.; Richard L. Garwin, IBM Corp.; L. Charles Hebel, Xerox Corp.; Richard A. Muller, University of California, Berkeley; Theodore B. Taylor, International Research and Technology Corp.; George F. Smoot, University of California, Berkeley, and Frank von Hippel, Princeton

University.

5. J. A. Forbes et al., "Cooling Water," *Environment*, 14 (Jan.-Feb. 1972); 40; D. F. Ford and H. W. Kendall, "Nuclear Safety," *Environment*, 14 (Sept. 1972); 48; H.W. Kendall and Sidney Mogilewsky, *Preliminary Review of AEC Reactor Safety Study* (San Francisco, Ca., and Cambridge, Mass.: Sierra Club and Union of Concerned Scientists, Dec. 1974); H. W. Kendall, *Nuclear Power Risks: A Review of Report of APS Society's Study Group on Light Water Reactor Safety* (Cambridge, Mass.: Union of Concerned Scientists, June 1975).

6. See, for example, Primack and Frank von Hippel, *Advice and Dissent: Scientists in the Political Arena* (New York: Basic Books, 1974), chap. 15; and Primack, "Nuclear Safety Controversy," *Chemical Engineering Progress*, 70 (Nov. 1974); 26.

7. S. AEC, "Theoretical Consequences of Major Accidents in Large Nuclear Power Plants," WASH-740 (Washington, D.C.: The Commission, 1975).

8. The minutes and records of the 1964-1965 WASH-740 Update were made available by the AEC in 1973. See Kendall and Mogilewsky, *Preliminary Review of AEC Reactor Safety Study*, p. 24 and Appendix C.

9. A. To Choose America's Energy Future: Final Report by the Energy Policy Project of Ford Foundation (Cambridge, Mass.: Ballinger Publishing Co., 1974); see also *Exploring Energy Choices: A Preliminary Report* (Washington, D.C.: Ford Foundation Energy Policy Project, 1974), especially Section 7.



A view from the outside

Fred C. Finlayson

The safety of nuclear power production and, specifically, nuclear reactors, cannot be conclusively and quantitatively established. Consequently, the assessment of nuclear reactor safety is largely a judgmental issue. Differences in value judgments account primarily for the polarization of elements in the technical community on the issue.

Because the evidence for reactor safety is largely qualitative in nature rather than quantitative, it has not been possible in the past to resolve nuclear safety issues to the satisfaction of the majority of the technical community, when the approach has been based upon a simple majority of the physical evidence. Experience has shown that resolution of safety issues has only been possible when the conclusion is supported 'beyond a reasonable doubt' by the available evidence. I will discuss reactor safety issues in general and some of the mechanisms which have been used in attempting to resolve them, using the emergency core cooling system as a specific example of an issue for which a completely acceptable solution has not as yet been achieved. Finally, I will discuss some of the mechanisms by which the evaluation of reactor safety could be made more quantitative and less judgmental so that uncertainties in reactor safety could be greatly reduced.

The principal past and current issues of public concern in reactor

Reactor safety is supported by qualitative evidence; more quantitative results are sought through the strengthening of systems evaluation, unremitting regulatory control and industrial incentives for greater safety

safety and the mechanisms for resolution of these issues are listed in Table 1. Some of the issues have been resolved, others have not. At least partial resolution has been obtained for problems of low-level radiation, site-related environmental issues, the potential conflict-of-interest between the developmental and regulatory aspects of the Atomic Energy Commission, and emergency core cooling system (ECCS) performance.

In considering those issues which were resolved, to a greater or lesser degree, it appears to me that when adequate solutions were found, they generally overwhelmed the problem. For example, the AEC's 'as-low-as-practicable' decree—limiting low-level radiation from normal plant operation to less than 5 millirems per year at the plant fence line compared to the natural background radiation received by the public which averages 100 millirems per year—was conceded by nearly everyone to clearly resolve this problem. Similarly, the AEC's response to the court's decision in the *Calvert Cliffs*' interpretation of the National Environmental Policy Act—wherein they accepted their NEPA responsibilities to prepare an adequate environmental impact statement for each site to be licensed—was clearly satisfactory to the majority of the citizenry. The developmental and regulatory as-

pects of the AEC were separated effectively by the congressionally mandated formation of the Energy Research and Development Administration and the Nuclear Regulatory Commission.

In the case of the attempted resolution of the ECCS issue, however, the results of the public rule-making hearings were not as satisfactory as those of the other issues mentioned above. Though many of the issues which were raised by the intervenors were resolved with the publication of the AEC's revised ECCS Acceptance Criteria, there were still several areas where the adequacy of the solution was disputed. The public rule-making hearings on the ECCS issue led to results which were clearly different in philosophy from those results which produced resolution in the other issues cited. For the ECCS issue, a compromise solution was sought, which espoused only a moderately conservative treatment of reactor performance in a loss of coolant accident (LOCA), instead of a strongly conservative position. As a result, a feeling of uncertainty persists.

The emergency core cooling systems are intended to prevent serious damage (especially core meltdown) to the reactor in the event of a loss of coolant accident. This type of accident may occur as the result of a break in any pipe in the reactor primary coolant system—up to and including the relatively improbable double-ended 'guillotine' break of the largest pipes in the system, which are about one-meter in diameter. In a reactor in which the emergency core cooling system performs properly after such an accident, peak temperatures are predicted to be of the order of 2,000°F approximately one to two minutes after the

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accident occurs, during the refill period. This is much hotter than the 600° F normal operating temperature of the core, and it is uncomfortably close to the temperature range where oxidation would become a significant contributor to cladding embrittlement.

Much of the problem with high temperatures during a loss of coolant accident occurs because reflood rates for pressurized water reactors (PWRs) are substantially lower than they were initially estimated to be. The original reflood design conditions for these reactors were predicted to provide rates of fluid rise in the core of the order of 10 inches per second. When more careful analyses of reflood rates were made, predicted rates had to be lowered to the current estimated maximum rates of about one inch per second as a result of the phenomenon of steam binding in the unbroken loops of the primary system. In the simplest of terms, steam binding occurs when rapid boiling of the refill fluid in the core causes droplets to be "carried-over" into the hot steam generators of the primary circuit and results in their being flashed to steam. The steam develops enough resistance to flow in the system to increase the internal pressure above the core and thereby reduces the reflooding rate of the core cooling liquid.

A reflooding rate of one inch per second is uncomfortably low for the cooling processes of reflood heat transfer. If this were not bad enough, the problem of steam binding could be seriously exacerbated by the potential failure of a small number of the literally thousands of individual tubes in the steam generator. If the tubes fail, steam from the secondary side of the system would be driven into the depressurized primary side, thus increasing the steam binding problem. If as few as about 10 tubes fail, it has been predicted that reflooding may not be possible at all in a pressurized water reactor. Though this may represent an oversimplification of the ECCS problem, steam binding and steam generator tube failure, resulting in a low reflooding rate, are dominant problems to successful ECCS performance.

In order to properly understand the background of the ECCS public

rule-making hearings, it is helpful to understand a little of the history of the ECCS controversy. Emergency core cooling was first seriously considered in 1966 when the Ergen committee—a task force of 12 engineers and scientists from the AEC and reactor vendors—was organized. The committee was formed as a result of AEC concern about the problems inherent in extrapolating reactor designs from the 100 megawatt (electrical output) units, which were then being built, to the 1,000 megawatt plants, which were then being proposed in order to capture significant economies of scale. As a result of the Ergen committee deliberations, a report on "Emergency Core Cooling" was written in 1967 which described the basic problem

The lengthy period required to bring the LOFT program to fruition has placed the U.S. reactor-safety program in a very difficult position.

and recommended a research program to resolve the issues.

A small test program called the Loss of Fluid Test (LOFT) was then under design to investigate the physical phenomena associated with core melt-down. As a result of the Ergen committee recommendation, the objectives of the LOFT program were redirected. The proposed LOFT studies of core meltdown effects were eliminated and were replaced by an investigation of ECCS design and performance.

The actual redesign of the LOFT reactor and facility was initiated in 1969. Since the changes in LOFT objectives occurred, substantial and continuing delays have plagued the program. The first delays were the result of the changes in the design concept which essentially required restarting the program. Subsequent delays have been frequently attributed to the experimental facility becoming a showcase for quality control procedures, as quality control began to be emphasized for commercial power plants by the AEC. This reason for delay was really in-

excusable, since there is no obvious technical reason why an experimental facility should be required to meet the same quality control standards required for commercial power plant operation.

As a net result of these serious program delays, the first tests in the LOFT program (with only an electrically heated core) are expected to begin in November 1975, while nuclear tests are not expected to begin before December 1976. Thus, the first really pertinent tests from this most important LOFT program will begin nearly 10 years after the program was initiated. The lengthy period required to bring the LOFT program to fruition has placed the U.S. reactor safety program in a very difficult position.

To compound the problems, from 1967 to 1972 the light water reactor safety research budget declined yearly, from about \$31 million in 1967 to about \$24 million in 1972. Though the AEC fought to maintain or increase the safety R&D budget, it was the policy of the Congress, especially the Joint Committee on Atomic Energy, to transfer the safety R&D responsibility to industry. The Committee argued that the nuclear industry had become mature enough to resolve its own problems—given today's complex safety issues, it was evidently a view which was ill advised.

Fortunately, the climate for federal support of reactor safety research has greatly improved. For example, in fiscal year 1975, \$58 million has been appropriated for the light water reactor safety research budget. There is good reason to believe that the fiscal year 1976 budget request of \$80 million for reactor safety research will be approved at essentially the requested level.

Returning to the historical commentary, in 1970 the AEC developed their "Water Reactor Safety Program Plan" in which the emergency core cooling system was designated as one of the "very urgent, key problem areas." In 1971 the "Water Reactor Safety Program Augmentation Plan" described the emergency core cooling system as the "most urgent problem area" in the safety program. The augmentation plan also acknowledged that the

then available experimental data and analysis techniques were "not now sufficient."

The Semiscale Test

That this observation was indeed true was highlighted by the 1971 results of the 'Semiscale Blowdown and Emergency Core Cooling Project.' This series of tests was conducted on an apparatus which was an early (and somewhat less sophisticated) single loop predecessor of the current 1½ loop Semiscale facility. These early tests produced alarmingly unexpected results—essentially all the emergency core-cooling fluid which was injected into the simulated reactor pressure vessel was ejected out of the break along with the original coolant water.

A reasonable explanation has been given for the unexpected results of that test: the early Semiscale facility was really an inadequate model of a pressurized water reactor. It contained only a single loop heat exchange path for fluid flow in the system rather than the multiple path design of a commercial pressurized water reactor. Consequently, no unbroken loops remained after the break was simulated in the Semiscale apparatus. The pressure vessel

modeling was also poor and bore little resemblance to the fluid flow path of an operational system. Moreover, the core cooling fluid was injected directly into the bottom of the pressure vessel, beneath the simulated core, rather than into the fluid return pipe ('cold leg') of the reactor as is the case for most commercial pressurized water reactors.

Consequently, it was clearly inappropriate to extrapolate the test results for direct application to a commercial pressurized water reactor. The principal problem with the results was that they were *unexpected*. Pre-test analytical methods had not predicted the total loss of core cooling fluid experienced in the tests. Although after the fact calculations of the test by vendors showed 'reasonable' agreement with test results, the real problem shown by the Semiscale test was the basic weakness of the calculational methods used for ECCS performance evaluation.

The Semiscale test results evidently accelerated the activities of an AEC regulatory staff task force which was considering standards for ECCS criteria. The task force quickly issued the ECCS Interim Acceptance Criteria on June 29, 1971, for enforcement without the customary 30

to 60 day public comment period. Almost immediately the adequacy of the emergency core cooling system, and, specifically, the Interim Acceptance Criteria were challenged by intervenors. As a result, public rule-making hearings were convened on January 27, 1972. The hearings lasted until July 25, 1973, encompassing 125 days of testimony by AEC, vendor, and public interest witnesses. The proceedings filled 22,380 pages of recorded transcript, and included over 1,000 referenced documents of roughly equivalent length. As a direct outcome of the hearings, substantially revised Acceptance Criteria for the emergency core cooling system were issued by the AEC on December 28, 1973.

Principal ECCS Issues

The sheer bulk of the material generated by the hearings is so formidable that it essentially defies careful evaluation by an outside individual without the benefit of financial support. However, in Table 2, I have attempted to provide a capsule synopsis of the principal issues raised in the hearings and the adequacy of their treatment in the revised ECCS Acceptance Criteria. Where the resolution of the issue was incomplete, a crude qualitative index of the importance of the issue has been given, indicating whether the issue is first or second order of importance. The issues cited in the table were derived essentially from a list of problems prepared by the AEC's Advisory Committee on Reactor Safeguards. The committee acknowledged that the listed problems did not have solutions that could be demonstrated to be conservative at the time of their publication. The list was extensively cited by the intervenors during the hearings. Consequently, the degree to which the problems cited were resolved by the revised Acceptance Criteria deserves evaluation.

A discussion of the technical issues presented in Table 2, albeit greatly abbreviated, can be found in the "Report to the American Physical Society by the Study Group on Light Water Reactor Safety." I shall only observe here that the first-order unresolved issues are indeed important ones. The importance of re-

TABLE 1
HISTORICAL DEVELOPMENT OF ISSUES
IN REACTOR SAFETY

Dominant Issues of Past	- Thermal Pollution - Low-Level Radiation from Normal Plant Operations - Environmental Impacts • Specific Siting Related Issues - Separation of Regulatory and Development Aspects of Nuclear Power
Current Issues	- Reactor Accidents • Loss-of-Coolant Accidents/Emergency Core Cooling System Performance • Pressure Vessel Failure • Other Modes: Anticipated Transients without Scram; Electrical Failure • Insurance Coverage—Price-Anderson Act - Waste Disposal • Transportation Risks • Engineered Storage • Ultimate Disposal - Safeguards—Nuclear Theft - Sabotage - Plant Reliability - Liquid Metal Fast Breeder Reactor Safety/Economics

flooding rates has been alluded to previously. Flow blockage and redistribution within the core of both pressurized and boiling water reactors can have very serious consequences. Preliminary estimates of this effect have shown the potential for increases in peak fuel rod temperatures of from hundreds to thousands of degrees.*

ECCS Performance

The present U.S. reactor safety research program, in attempting to resolve the questions about ECCS performance, has thus far emphasized investigation of bits and pieces ('separate effects') rather than evaluation of system-level performance. Though the results of separate effects tests are important, they cannot replace system-level tests. The LOFT reactor with a design thermal power of 55 megawatts (thermal)—approximately $1/60$ scale of a commercial 3,300 megawatt (thermal) or 1,000 megawatt (electrical) pressurized water reactor—is the largest currently planned system-level test. Direct extrapolation of its results for application to commercial power reactors will not be credible. Instead, elaborate computer programs must be used to understand the implications of these tests for full scale reactors. Larger scale tests, perhaps on the order of one-quarter scale or larger, may be needed to establish the validity of such computer programs.

The validity of numerical methods for predicting accident consequences can only be conclusively demonstrated by comparison of their predictions with the results of system-level tests. Past experience with mammoth codes implies that it is improbable that the codes will predict early LOFT results adequately without 'maturation' of separate effects subroutines against the test results. Consequently, though the codes are potentially our strongest basis for qualitative evidence of reactor safety, it is highly probable that we will not be certain of their validity.

*W. R. Gambill, "Estimate of the Effect of Localized Flow Blockages on PWR Clad Temperatures During Reflood," *Proceedings of Topical Meeting on Water Reactor Safety*, Salt Lake City, Utah, March 26-28, 1973, pp. 221-240 (National Technical Information Service document CONF-730304).

ty on the basis of LOFT test results. We will be left with the nagging suspicion that the codes have been fine-tuned to LOFT results. With the several scaling compromises and design deviations from current pressurized water reactor practice which exist in the LOFT facility, we may then remain uncomfortable about a 60-fold extrapolation of the code predictions to the scale of commercial pressurized water reactors.

Perhaps an even more painful observation is the almost complete dearth of integral system test results for boiling water reactors. Essentially no large-scale system tests of the performance of boiling water reactors under abnormal conditions have been conducted. Some European tests have been conducted on a small-scale boiling heavy water (Marviken) reactor (140 megawatts electrical) which has no nuclear core and is heated by an oil-fired burner. The results are evidently of uncertain relevance for boiling water reactors in the United States.

Conclusions

The APS study group summarized its conclusions with respect to ECCS performance as follows:*

We have no reason to doubt that the ECCS will function as designed under

most circumstances requiring its use. However, no comprehensive, thoroughly quantitative basis now exists for evaluating ECCS performance, because of inadequacies in the present data base and calculational codes. In addition, it is not clear that the present approximate calculations, even though based on generally conservative detailed assumptions, will in all cases yield conservative assessments of ECCS performance.

We have examined the AEC reactor safety research program intended to resolve these uncertainties. Expanded experimental tests and advanced calculational code development are now under way, with the goal of accomplishing a sufficient quantitative comparison between calculation and experiment so that the technical community can reach consensus on ECCS effectiveness. That consensus can only be reached through several years of effort, using improved research techniques, and with more open publication and review of the results. We doubt that a complete quantitative evaluation of ECCS effectiveness can be achieved through the present program. We recommend below several possible approaches for improvement.

The ECCS safety margin should be quantified, and if necessary, improved through one or more of the following approaches:

*American Physical Society, "Report to the APS by the Study Group on Light Water Reactor Safety," *Reviews of Modern Physics*, 14 (1975), supplement 1, pp. S-6, S-7.

TABLE 2 ECCS HEARINGS ISSUES

Conservatism of Interim Acceptance Criteria Evaluation Model Elements
Challenged in Following Areas:

Issues Raised	Acceptance Criteria Resolution	Issue Significance
Analytical Models and Numerical Methods	Incomplete	0(1)*
Amount of Initial Stored Energy in the Fuel	Adequate	—
Treatment of Phase Separation and Nonequilibrium Effects	Incomplete	0(2)
Treatment of Loop Resistances	Adequate	—
Treatment of Hot Channel Flow, Including Flow Blockage and Flow Redistribution	Weak	0(1)
Treatment of Break Flows	Adequate	—
Treatment of Decay Heat	Adequate	—
Transient Critical Heat Flux and Heat Transfer	Adequate	—
Treatment of Clad Ductility	Partial	0(2)
PWRs: Distribution of Injected Water—Reflooding Rates, Reflood Heat Transfer, and Carryover	Weak	0(1)
BWRs: Level Swell—Spray Heat Transfer	Adequate	—

*0(1)—First order; 0(2)—Second Order

- the substitution of more easily analyzable or more effective ECCS concepts;

- a much stronger theoretical and calculational development effort combined with a much improved experimental program, the results of which must be published openly for evaluation by the technical community;

- a series of large-scale experiments along with some standardization of reactors. Detailed planning and analysis for this approach should begin immediately in case it should be decided in the future that it is needed. There should be increased emphasis on realistic calculations and experiments as opposed to those which merely attempt to set upper limits on the behavior of a reactor in an accident. In view of the number of reactors now operating and being planned, we believe it is important that the reactor safety research program quickly take major steps to bring about a convincing resolution of the uncertainties in ECCS performance.

I personally agree with these conclusions. I would like to emphasize, however, the basis for my own belief that the emergency core cooling system will function adequately, in most cases, in spite of the acknowledged inability to quantify the conservatism of its predicted performance. First, ECCS hardware is composed of redundant, indepen-

performance under multi-phase conditions, and the swelling and rupture of fuel cladding (to cite only a few representative cases) are weak.

The central issue with respect to the validity of the mathematical models and computational methods is associated with their lack of system-level confirmation. There is substantial uncertainty about the capability of the methods to adequately treat the conservation of momentum throughout the primary system, properly describe the effects of blockage and radial flow, and correctly predict the reflooding rates. Nevertheless, I believe the preponderance of the evidence supports the judgment that the emergency core cooling system will perform adequately, under most circumstances.

Thus, the general conclusions with respect to ECCS performance can be seen to have a heavy judgmental basis. It is impossible to give too much emphasis to the need to improve the quantification of the critical reactor phenomena and the overall system responses so important to reactor safety. The evaluation of reactor safety must not be permitted to remain so heavily dependent upon engineering or scientific judg-

formance with the spirit as well as the letter of regulatory criteria and standards.

With this in mind, the regulatory reactor design philosophy embodied by the current criteria and standards should be reviewed. The current approach embodied in the design-basis-accident (DBA) concept does not seem to adequately motivate vendors and utilities to seek out design improvements which could improve reactor safety.

It has been observed that under the design-basis-accident philosophy, safety features added to a reactor to improve conditions may simply result in redefinition of the design-basis-accident. For example, if a vendor/utility were to incorporate a core-catcher (or second containment building) into a new power reactor, the design-basis accident for the facility might simply be changed to encompass a worse accident than before—the failure of the new safety system. Moreover, utilities may not be given 'credits' for safety improvements. Utilities fear that design modifications may be treated in licensing procedures as troublesome deviations rather than safety improvements.

Thus, the design-basis-accident philosophy must be reviewed with an eye toward changes in the philosophical basis for reactor design which will offer positive inducements to reactor vendors and utility owners to exercise real initiative in improving safety. It would be most desirable to develop and implement an approach which would provide incentives to achieve greater system safety and also to motivate investigations (with public review) of costs and benefits of creative safety concepts.

Once the evaluation of reactor safety is conceded to be a judgmental issue, it is important to attempt to place the risks associated with nuclear energy production in perspective. The risks of reactor accidents will be addressed in length in other papers in this issue. I would only like to preface their remarks by an observation on the relative nature of safety.

When we question the safety of reactors, we must also ask,

Though nuclear energy production has the potential for infrequent but large-scale accidents with substantial consequences, I believe progress is being made in understanding and reducing the probabilities of accident occurrence.

dent elements which are at least nominally adequate in fluid capacity. The revised ECCS Acceptance Criteria for evaluating design performance are generally conservative in most of their elements. However, although the ECCS evaluation models based upon the criteria are consequently probably also conservative in most instances, the more 'realistic' numerical models of many of the loss of coolant accident phenomena are inadequately formulated. For example, models of multi-phase (steam-liquid) flow, pump

ment. To this end, the recommendations of the APS study group, previously cited, should be implemented as rapidly as possible.

Reactor Design Philosophy

Regulatory control of reactor design, construction, and operation should be strengthened. Since the judgmental basis for evaluating reactor safety now depends so heavily upon maintaining or improving the quality of performance in each of these areas, we must be increasingly vigilant to assure vendor/utility con-

"Compared to what?" Various estimates have been given of the average number of persons expected to be killed per year due to reactor accidents. These numbers are on the order of one person per year and are obviously much smaller than the approximately 1,200 people per year in the United States that die from electrocution, the 200 people that die from accidents in commercial air travel or a similar number that die from natural hazards such as lightning, earthquakes, etc. It is also a far smaller number than the 4,000 people in the United States who are estimated to die each year from complications attributable to natural background radiation.

Expressed on an annualized basis in this way, it is unlikely that a risk of death of something on the order of one person per year would be of grave concern to the public. It is, however, the potential for taking a large number of lives with a single very rare accident, perhaps on the order of 10,000 lives, and contaminating large areas of land for years which changes the relative concern which the public feels for the problem—no matter how infrequent the accident may be. Few other man-made things have this potential for such large-scale disastrous consequences. Earthquakes, hurricanes, and famines are the only relatively common sources for disasters where

thousands of lives are at risk from a single event. In my opinion, it is this potential for large-scale catastrophe, even though extremely infrequent, which motivates the concern of the public. There seems to be a psychological limit to the maximum number of deaths from a single man-made event which can be tolerated—and reactors are suspected of being capable of approaching that limit.

In conclusion, I believe the judgmental balance of evidence favors the continued development of nuclear power. Though nuclear energy production has the potential for infrequent but large-scale accidents with substantial consequences, I believe progress is being made in understanding and reducing the probabilities of accident occurrence. Moreover, there is evidence to believe that work can, and will, be done to mitigate the consequences of accidents. If this work is diligently pursued, progress should be made at rates which are compatible with the current growth rate of nuclear power in the United States so that the overall risk to the public should not increase and should, in fact, decrease. Under these circumstances, I personally believe the risks are acceptable, compared to the risks associated with alternative modes of energy production.

To achieve the goal of continually

reducing the risks related to nuclear power production, we must be sure that the past practice of the Atomic Energy Commission and the Joint Committee on Atomic Energy of minimizing the appearance of the risks and dismissing the legitimate questions of the public is not repeated. We all owe a substantial debt of gratitude to several technically competent, diligent intervenors for raising the level of the discussion of nuclear safety issues and standards. Conversely, we have seen nuclear reactor antagonists who seem dedicated to disruption and intransigence. The seriousness of the energy situation in the United States calls for open-minded, positively directed public participation, not for disruptive tactics. To accomplish this, our newly formed Nuclear Regulatory Commission must continue to recognize its responsibility to the public and, with the continuing support of Congress, actively encourage public participation in the evaluation of nuclear safety issues and support public dissemination of reliable information on these issues. If we can accomplish this, I believe that nuclear energy can be utilized with adequate, and increasing, safety to the public and should be allowed to achieve its anticipated, important role in the overall U.S. energy program.

The safety study and its feedback

Norman C. Rasmussen

In August 1974 the results of the Atomic Energy Commission's Reactor Safety Study were issued in draft form as document WASH-1400. This report was a quantitative risk assessment of nuclear power plants of the type being put into service in the United States today. It was the result of about 70 man-years of effort. The draft was widely circulated for comment to interested parties, and the comments received now exceed the 3,300 pages of the draft

The principal investigator reviews his report in the light of criticism and comment; he finds that the basic conclusions still stand

report itself. These comments are being carefully reviewed, and the many valid suggestions they contain will be used to improve the draft before it is issued as a final report sometime this fall. This paper will discuss some of the principal comments and criticisms and the current status of the report.

The study group was faced with two major tasks: (1) to identify those accident sequences with potential public consequences and estimate their probability of occurrence, and (2) to calculate the consequences of various size releases of radioactivity resulting from the identified poten-

Norman C. Rasmussen, nuclear engineer and head of the Department of Nuclear Engineering at the Massachusetts Institute of Technology, directed the AEC's Reactor Safety Study, known as WASH-1400.

tial accidents. The techniques used in these tasks are described fully in WASH-1400 and so only the basic principles will be described here.

Two logic techniques, called event (or sequence) trees and fault trees, were the basic tools used in task 1. *Event trees* are a form of decision trees that have been widely used in the field of decision analysis for a number of years.* In the case of accident analysis they start with a particular initiating event (for example, a pipe break, a condition requiring shutdown, etc.) and project all the possible outcomes of that event. The various outcomes depend on the operability or non-operability of all those plant systems which can affect the course of events. In a reactor these are the systems that can prevent fuel melting and, if the fuel melts, those additional systems that can affect the size of any radioactive release. Since fuel melting is the condition of particular concern it is necessary to identify those initiating events which could potentially result in fuel melting, and to draw an event tree for each.

Two general classes of initiating events were considered and defined: The first are, those events in which the coolant was lost as a result of a failure in the primary system boundary, *loss of coolant accidents* (LOCA). Whether or not the core melts in these cases is determined by the effectiveness of those emergency core-cooling systems designed to cope with such failures. The second class, called *transients*, consisted of those events, planned or unplanned, which lead to a signal to shut down the reactor. In this latter case it is usually necessary for the reactor shutdown system to work and at least one of the cooling systems must operate to remove decay heat to prevent core melting.

In the event, say, of a pipe break accident, if the probabilities for successful operation of the various important systems were known, it would in principle be possible to calculate the probability of each of the various accident sequences. Clearly such a calculation is not that

simple, because it is not likely that the probabilities are independent as assumed by their simple multiplication. This is known as the *common mode failure problem*. Thus, it is necessary to carefully search for dependencies between those probabilities that are multiplied together. Any identified dependency can readily be accounted for by using well-known statistical techniques. A considerable amount of effort went into identifying such dependencies.

The accident sequences of the event trees define the condition of the reactor systems. This allows calculations of the physical state of the system, including whether or not the core melts. If it melts, a reasonable estimate can be made of how much of each radioactive species is released. If the probabilities are

Although it would be highly desirable to have better failure rate data so the uncertainties could be reduced, the results obtained were useful as a risk estimate, nonetheless.

known, it is possible to generate a histogram of probability of release versus release magnitude. This histogram becomes the source term for consequence calculations.

The event tree does not provide any way of determining the probabilities defined by the various branch points. To obtain these probabilities, the second logic technique, *fault trees*, was employed. In this technique one starts with a particular undesired event (for example, failure of electric power to the emergency systems following a pipe break) and then reasons backward to identify what component failures or combinations of component failures might have led to this system failure. Since there is considerable data available on the expected failure rate of the various components (pumps, valves, relays, switches, etc.), this data can be used to predict the system failure rate using the fault tree logic. The failure rate data on these various components comes both from experience in nuclear power plants and in other commer-

cial industries using these components under similar operating conditions. There is, of course, statistical uncertainty in all such data and this uncertainty was propagated through all calculations.

The fault trees were used to calculate the failure rates of various systems including the emergency core cooling system, the electric power system, the fission product removal system, etc. Some 10 to 15 systems for each type of plant were fault treed. However, the probabilities of such single events as a large pipe break are not amenable to such techniques and their probability was determined from operating experience.

Criticisms and Comment

Almost all commentators agreed that these techniques were very useful in understanding the relationship of various events in an accident. Most also felt that they could be used to provide meaningful relative values of the probabilities. However, some felt that it was not possible to predict absolute probabilities using these methods. This skepticism was mostly based on concern for one or more of the following reasons: (a) It is unlikely that all possible accident sequences have been identified. (b) The failure rate data is not well enough known. (c) It is unlikely that all the dependencies between systems and components have been identified and dealt with properly—for example, the common mode failure problem. (d) The contribution of human actions is impossible to quantify and treat properly.

Each of these criticisms is worth examining. There are examples in some of the early work in the reliability field where failure to deal with them properly led to unrealistically low predictions of system failure rates.

It is almost certain that not all possible accident sequences have been considered. The analyst generally will postulate only those kinds of failures that have been experienced. Clearly, it is likely that types of failures exist which we are not aware of. However, it must be remembered that the objective of this analysis is to predict the failure rate with useful accuracy. For risk analy-

*H. Raiffa, *Introductory Lectures on Making Choices under Uncertainty* (Reading, Mass.: Addison-Wesley, 1968).

sis an uncertainty factor of ± 5 is useful. In WASH-1400 the uncertainty range was generally about a factor of 20. To achieve this accuracy it is only necessary that the dominant contributors to failure be considered. Clearly all sequences that are a factor of 10 (or more) smaller in probability than other sequences that produce the same consequence can safely be ignored in terms of their effect on the risk estimate.

Since there have been many observed failures in almost all types of components in these systems, by definition the most likely failure modes have been observed. If the analyst has been careful to include all these failure modes in the analysis, then the dominant contributors to failure have been considered and only very unusual or rare types of failures have been omitted. But even if included, they would not change the overall system failure probabilities significantly.

In the case of *failure rate data*, it is true that it is not accurately known in many cases. The range of statistical uncertainty was found in most cases to be a factor of 10 (that is, $\pm$ a factor of 3) in the best cases, and a factor of 100 or more in cases where less data was available. In addition, it was often difficult to determine what types of distribution characterized the data. The study group found that the data in all cases was quite consistent with a log normal distribution and this distribution was used in all cases. Because of the rather large uncertainty in some of the data the common methods for propagation of error were often not valid, so a Monte Carlo method was used to develop the distribution of system failure probability.

The final result of the propagation of these uncertainties did lead to considerable uncertainty in the answers. Generally this fell between $\pm$ a factor of 3 and $\pm$ a factor of 10 for various systems. The final risk curves were estimated to have a range of uncertainty in probability of a factor of 20.

Although it would be highly desirable to have better failure rate data so the uncertainties could be reduced, the results obtained were useful as a risk estimate, nevertheless. As noted in the draft report, the

differences between certain nuclear risks and other common risks were so large that some meaningful conclusions could be reached.

The *common mode failure problem* is a very difficult one to treat. However, there are many cases where it is not as important as intuition might suggest. For example, in cases where a single component failure can cause system failure, common mode failures are rarely very important. Of course, such systems will typically have quite high failure rates. On the other hand, in highly redundant systems, such as the reactor shutdown system, common mode failures become very important.

To illustrate this, consider the reactor shutdown system which the study found to be triply redundant.

The worst consequence of a nuclear accident dealt with in this study had a probability per plant per year of one in a billion. Some critics have argued that such small probabilities are unrealistic.

Each individual failure had a probability of failing on demand of about 10^{-3} (one chance in 1,000). Thus, on the basis of independent failures the probability of failure on demand would be about 10^{-9} (one in a billion). After including the identified dependencies between failure events the study obtained a failure rate for these systems in the range of 10^{-4} to 10^{-5} (one in 10,000 to 100,000) per demand. Clearly in this case common mode failures were the dominant contributor.

A variety of techniques were used in the study to determine the possible common mode failures. Every attempt was made to include the effect of common environmental factors and operator and maintenance errors that might affect several systems at once. Not surprisingly, in many of the systems we found these effects to be important contributors to the overall failure rate determined.

A variety of *human factors* were also considered in the analysis. These included errors of omission or

commission by plant operators and maintenance personnel. There is data available on human failure rates although it generally has a rather large uncertainty, typically about a factor of ± 10 . Human failures were included and treated in the same way as equipment failures in the failure rate calculations.

There has been considerably more experience with the use of these techniques for predicting human failure rates than many people realize. Probably the most experienced user of these techniques is the Systems Reliability Service of the U.K. Atomic Energy Authority. This group consists of more than 100 professionals who routinely apply these techniques to a wide variety of commercial and military systems. It was organized and developed by F. R. Farmer, one of the pioneers of these methods. The group's experience in the prediction of failure rate on some 50 different commercial systems is reviewed in *Reliability Technology*, which was written by two members of the group.* These predictions, which were made prior to service of the equipment, were later compared to the experience. The observed failure rate was found to be within a factor of ± 40 of the predicted rate in 96 percent of the cases. On the average, the observed failure rate was 76 percent of the predicted failure rate; thus the predicted failure rates were about 30 percent greater than the observations on average.

This experience clearly shows that if techniques are carefully applied by trained people absolute predictions can be made with reasonable accuracy. It should be pointed out that NASA has used these techniques for a number of years; but not to predict the overall absolute probability of mission success. Their case is a much more difficult problem, since they use hardware that is quite different from common commercial hardware on which the failure rate data is based. In addition, their systems must operate in environments in which we have very limited experience. Such factors, of course, tend to make absolute probability

*A. E. Green and A. J. Bourne, *Reliability Technology* (New York: Wiley-Interscience, 1972), chap. 13.

calculations much less certain.

To date there are about 250 plant-years experience with large nuclear power plants in the United States. This, of course, is not enough to establish that the AEC's study achieved the same good results that the British have shown. Nevertheless, it should be noted that the AEC report predicted the failure rates for about 20 different reactor safety systems. The failure rate on demand for all these systems fell in the range of 10^{-2} to 10^{-5} (one in 100 to 100,000). These numbers are quite consistent with what the British find for a wide variety of well-designed safety systems. Thus the results presented in WASH-1400 seem consistent with engineering practice in a variety of nuclear and non-nuclear applications.

Probability Consequences

The worst consequence of a nuclear accident dealt with in this study had a probability per plant per year of 10^{-9} (one in a billion). Some critics have argued that such small probabilities are unrealistic. Let us consider how this number comes about. The worst accident is one that involves an initiating event followed by the worst set of system failures designed to cope with this event. This produces the largest radioactive release. To produce the largest consequence, this must occur under the most stable weather conditions with the wind blowing toward the highest population density. Thus the probability per year is:

$$\left[\begin{array}{c} \text{Probability of} \\ \text{worst} \\ \text{consequence} \end{array} \right] = \left[\begin{array}{c} \text{Probability of} \\ \text{initiating} \\ \text{event} \end{array} \right] \left[\begin{array}{c} \text{Probability of} \\ \text{safety} \\ \text{system} \\ \text{failure} \end{array} \right] \left[\begin{array}{c} \text{Probability of} \\ \text{highest} \\ \text{population} \\ \text{density} \end{array} \right] \left[\begin{array}{c} \text{Probability of} \\ \text{worst} \\ \text{weather} \end{array} \right]$$

$$[10^{-9}] = [10^{-3}][10^{-3}][10^{-1}][10^{-2}]$$

Thus, the 10^{-9} (one in a billion) is made up of 4 factors in this simplified example and whether it is believable depends upon how independent one feels the various probabilities are. Although one can imagine possible dependencies between some of the terms, these dependencies are surely not very strong. For

The worst imaginable consequence of the worst nuclear accident is a very unlikely event.

example, one can imagine weather that could cause the initiating event, but it surely occurs much less than 10 percent of the time. In addition, such weather is not highly stable with low wind velocities needed to produce the worst consequence. Thus I believe that in the vast majority of the accidents it is reasonable to assume that the weather, the population density, the initiating event, and the safety system operation are independent. Although one can postulate events where certain dependencies will exist, they are so rare that they are not likely to affect the overall probability very much.

Basically all the above argument says is that, given an accident, the worst imaginable consequence is a very unlikely event because it requires the simultaneous occurrence of a series of independent, unlikely events. A study of the history of accidents in a variety of fields shows that this is true and it has been pointed out by authors of other risk studies.

The consequence model in the WASH-1400 draft report received a much smaller fraction of the project effort than the task just discussed. This was because at the outset it was felt that this was a much easier part of the problem. As the study progressed, however, it was discovered that this was a much more difficult problem than first realized. In order to meet the deadline for issuing the draft a number of simplifying assumptions had to be made. Some of these are noted in the draft as being unrealistic. Since issuing the draft, considerable work has gone into improving this model to be more realistic.

Among the comments received have been some that pointed out that a few of these assumptions were not consistent with the best available data. In addition, there were a

few mathematical errors discovered in the code. The final version of the code is now nearly complete and includes a number of improvements to make it more realistic as well as the corrections and improvements that have been discovered or pointed out. The principal changes will include an improved meteorological model, evacuation model, biological conversion factors, and property damage model.

The area where the largest change will occur at this point appears to be in the calculation of the long-term population dose, principally as the result of the isotope cesium-137. Several commentators, including the American Physical Society, pointed out that the draft report of WASH-1400 had underestimated the effects of this isotope. The APS, on the basis of a simplified but quite good model, has suggested that these long-term population doses may be underestimated by a factor of 3 in the worst case and by a factor of 25 in the average case.

Preliminary results indicate that the factor of 3 in the worst case may be about correct, but the factor of 25 is too high by a factor of about 5. This is not due to any error in the APS calculation, but to the use of more realistic numbers of certain parameters (particularly the population density at long distances) than those stated in the draft report which the APS used.

It now appears that the overall risk curves for the various consequences are not likely to change by much more than the factor of ± 3 assigned as the uncertainty in the draft report. Those effects related to long-term population dose will, however, increase by about a factor of 3.

The many valuable comments received on the draft of WASH-1400 will result in a much improved final report, particularly in the consequence model. At present it is anticipated that there will be no major change in the accident sequences and probabilities part; but there have been many suggestions that will lead to a clearer, more understandable discussion of that part of the work. It is anticipated that the final report will be released sometime this fall.



Virtues and limitations of risk analysis

Robert K. Weatherwax

The Reactor Safety Study of the Atomic Energy Commission (the Rasmussen report) represents a quantitative evaluation of the hazards associated with the current generation of light water reactors. This evaluation features a comprehensive application and, in certain instances, an expansion of risk techniques developed for aerospace applications. Using these techniques a systematic evaluation of certain reactor designs was performed to identify potential accidents, and to project the consequences of the most serious accidents. Significant results of the study included identification of weakness in the reactor designs evaluated, estimation of the relative impact of individual accidents on risk, and estimation of the probability of occurrence of different accident consequences.

The summary and the main document of the Rasmussen report stressed the cumulative risk predictions and almost completely ignored the results which are of more practical engineering importance. This emphasis is exemplified by Figure 6.1 which appears in the Rasmussen report (see p. 39). This figure presents the predicted risk of prompt fatalities from nuclear accidents in comparison with the observed risk from other hazards to modern man. No indication is given of the relative level of uncertainty for the risk curves although the uncertainty range for nuclear risk is much greater than it is for the other actuarially determined risks. The great predicted spread between the risk level for the first 100 large nuclear power plants and the levels identified for the other hazards—in conjunction with the lack of any warning about the comparability of the risk data—

A significant step forward in the assurance of reactor safety, but an imperfect measure of actual reactor risk

has encouraged nuclear power partisans to claim that the results validate their position that commercial nuclear power plants are much safer than other human endeavors. Thus, the manner in which the results have been presented in the Rasmussen report places a great emphasis upon the value of the effort as an accurate estimator of the risk attendant to the first generation of power reactors, and discounts the utility of the effort as an engineering tool for reactor safety optimization.

This choice of emphasis may prove to be unfortunate. Traditionally, risk analysis efforts have been performed with the objective of identifying and eliminating or moderating the greatest contributors to risk. Adequate techniques and data have never been available to produce accurate estimates of absolute risk. The AEC's study is more comprehensive than any other study performed to date of which I am aware, but it is sufficiently similar to other efforts to question the accuracy of its absolute risk assessments. Regrettably, if the assessments are later shown to be inaccurate, there will be a tendency to dismiss the study as worthless, even though its value as a tool for safety design optimization will remain.

In the remainder of this paper I would like to review the probabilistic portion of the Rasmussen report from the perspectives of engineering utility and risk assessment uncertainty. Drawing upon experience derived from previous risk analyses and the actual approach implement-

ed in the Rasmussen report, I will endeavor to show that the report may represent both a significant step forward in the assurance of reactor safety, and an imperfect measure of actual reactor risk.

The methodology applied in the Rasmussen report comprises a logical integration and extrapolation of fault tree analysis and sequence (or event) tree analysis. These analyses have been developed and used for prediction of risk associated with aerospace systems.¹ Fault tree analysis is a methodology for logically structuring the unions and intersections of component failure modes within a complex system (for example, a nuclear weapons system) in order to identify the sequences of component failure modes that have the effect of causing a specified system-wide failure (for example, the inadvertent detonation of a warhead). The probability of each identified failure sequence is estimated along with the predicted probability of the specified catastrophic event resulting from all potential failure sequences. The value of the fault tree approach is that it provides a methodology for predicting the frequency of unlikely events by reducing them to constituent failures for which failure data is available.

Sequence tree analysis is a much broader methodology that uses a probability tree format to predict the expected number of casualties resulting from complex initiating accident-equipment response sequences. Its principal application

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has been in predicting risk levels for launch and operation of space nuclear power systems: initiating accident and nuclear system response probability estimates are combined to predict radioactivity release mode frequencies and, then, these are combined with the number of resulting casualties—determined by radiological analysis—to produce risk plots analogous to those presented in Figure 6.1 of the Rasmussen report (p.39).

In fault tree and sequence tree applications in the United States to date, relatively little importance has been attached to the estimates of absolute risk. The emphasis has been on estimating the relative likelihood of failure sequences, and identifying the steps necessary to reduce risk through evaluation of the largest contributors. Great confidence is felt in the ability to identify the highest risk items, and to choose the lowest risk configurations from alternate design options. Due to the extreme improbability of the accidents addressed by these analyses, there is no way to judge with statistical precision the accuracy of the absolute estimates of risk. However, from the observed frequency of lesser accidents, it appears that several orders of magnitude of uncertainty may be present, depending upon the predicted rarity of the accident.

The probabilistic portion of the analysis in the Rasmussen report was accomplished through the application of both of these logic techniques. Sequence tree analysis was used to develop the sequences of engineered safety features and containment equipment failures that would result in release of radioactivity. Fault tree analyses were developed to predict the probability of occurrence of each identified combination of safety system failures. Thus, the methodology applied in the Rasmussen report featured the overall breadth of the sequence tree approach in combination with the ability of fault tree analysis to predict rare events in complex systems. These combined techniques then have the ability to identify design weaknesses and optimize safety but are limited by the uncertainties inherent in each of the techniques. This needs to be borne in mind as

we review the accomplishments and limitations of the Rasmussen report.

To my mind the most positive aspect of the Rasmussen report was the non-technical consideration that it represented an official acknowledgment that nuclear reactors can fail catastrophically, and that they need to be exposed to detailed engineering review. Technical strengths of the report, unsurprisingly, reflected the fact that a probabilistic risk analysis had never been previously performed on commercial nuclear power reactors.

The relative difference in probability of core melt between the boil-

The most positive aspect of the Rasmussen report was that it represented an official acknowledgement that nuclear reactors can fail catastrophically, and that they need to have detailed engineering review.

ing water and the pressurized water reactor designs analyzed was estimated by the Rasmussen group. Even if one does not take the absolute estimates to be accurate, the difference of only a factor of two in probability is indicative of similar risk levels associated with each reactor, and that is certainly new information.

The failure frequency ranking of important core melt failure sequences has provided an invaluable focus on design areas requiring additional emphasis. The realization that reactor 'transients' and $1/2$ inch to 2 inch primary coolant system pipe ruptures are the most important initiating accidents is significant because previously it was thought that large pipe ruptures were the most significant hazard. The capability of the methodology to find subtle failure interdependencies is graphically established by the number of identified common modes which greatly reduced the effectiveness of the engineered safety features.

The easily correctable design weaknesses that were identified—like the common mode failure of the connecting valves between the pri-

mary coolant system and the low pressure injection system of the pressurized water reactor—should be, and may have already been, modified in existing reactors, thereby increasing reactor safety. Other deficiencies which would require significant design modifications will need to be more carefully reviewed in the light of cost impact and the relative frequencies of failure vis-à-vis other equipment. Thus, this analysis has permitted safety questions to center on rational cost-benefit trade-offs instead of relying upon purely subjective judgments. Since the methodology does represent the best contemporary approach to reactor safety improvement it should be of continuing value and applicability.

Certain procedures and approaches used in the Rasmussen study appear to raise questions as to the accuracy of the probability estimates stated in the report. These items, which are presented in the table, are divided into those which appear correctable within the analysis ('weak points') and those which appear unavoidable from a methodology limitation perspective.

Analysis Weak Points

This analysis drew upon failure data from all light water reactors but only considered the specific design details of one pressurized water reactor and one boiling water reactor. Differences between these reactors and the other reactors in commercial operation were ignored. Also, the Rasmussen report did not consider the typical phasing of power plants: one unit is built, made critical, and put into commercial operation while a companion plant is being built. Safety is very likely to be degraded during the construction period. The overall impact of these factors on risk is likely to be significant and is not reflected in the results.

The Rasmussen report addresses itself to human error in regard to control room and active maintenance failures, including failures under emergency conditions, but it does not consider the prospects for failure to perform monitoring and maintenance at the frequency specified or to obey operating rules for shutting the reactor down when

necessary whether due to human error or malfeasance. The sensitivity of the final results to incidents of this type should have been measured, as they may well be significant.

It is my understanding that uncomputerized Boolean (algebraic) reductions were used extensively to identify failure sequences. This was apparently necessary because of the extremely large fault trees produced during the study that exceeded the capabilities of the fault tree quantification computer program used in the study.² Perfect Boolean reduction is necessary in order to identify common failures in apparently independent equipment. Past experience has shown this procedure to be extremely error prone, and to become progressively more so as the size of the fault tree increases. Thus, it is questionable in my mind whether the fault trees were properly reduced and all common failures identified. Other codes exist which could have eliminated this problem and should have been used.

At each level of the analysis a log-normal distribution of failure rate data was assumed with 5 and 95 percentile limits defined. Considering the typical spread of two orders of magnitude between these percentile levels, this resulted in the mean for each failure distribution being approximately three times as large as the median which was used in the point computations. Theoretical arguments do not justify assuming the spread in failure rate data obeys a log-normal distribution, nor does a cursory inspection of the data plots presented in the Rasmussen report support such a contention. A curve-fitting program should have been used to define the distributions, or the insensitivity of the magnitude of the risk to use of the log-normal distribution should have been clearly established.

The failure rate data and discussion of their use do not permit one to evaluate (1) how the nuclear data was used to adjust the non-nuclear data, (2) how the 5 and 95 percentile values were established for each component, or (3) what adjustments were made to the failure rates to reflect the severe radiation and stream environments resulting from a core melt. Without this information

it is impossible to trace through the risk calculations.

In addition to these specific sources of uncertainty and error which are capable of resolution within the analysis framework, other sources of uncertainty inherent in the risk methodology exist which make it very difficult to compute an accurate absolute risk prediction. Foremost in this category are questions of availability and appropriateness of failure rate data. Obvious examples of this problem are (1) the probability of reactor vessel failure, (2) the increase in component failure rates associated with post-loss-of-

Major Contributions to Uncertainty of Probability Estimates in Rasmussen Report

analysis weak points

Failure to review other power reactor designs and to reflect serial construction

Failure to consider procedural violations except under stress

Capability for identifying common modes and Boolean algebra errors

Use of the log normal distribution to reflect failure data spread

Failure data normalization

methodology limitations

Absence of appropriate failure rate data

Assumptions of constant failure rates

Inability to anticipate and quantify specific human errors and failure dependencies

Design deficiencies

coolant-accident environments, and (3) error rates for operating personnel. It is certainly possible to develop relative failure likelihoods for these events; but it is extremely difficult to acquire sufficient data to establish values within a reasonable degree of statistical confidence.

The exponential distribution is assumed for all component failure rate data. This is equivalent to saying that each component will only fail at a threshold stress which has an equal probability of occurring throughout the life of the component; and that previous stress levels, since they did not exceed the threshold level, have no influence on the constant instantaneous failure rate. This assumption is almost universally used in reliabil-

ity computations and is quite applicable over restricted regimes.³ However, most components age and eventually enter a wearout regime distinguished by increasing instantaneous failure rate. Since these plants are designed to endure for 30 to 40 years it appears extremely likely that age will take its toll, with an attendant increase in risk level.

The results of the Rasmussen report are dominated by single failures, most of which involve identified human error. Clearly, it is a difficult task to quantify the probability of these identified failures; however, that is an easy task compared to the need to anticipate every potentially significant negligent and/or erroneous act that will occur during the life of the plant. This is especially true when one views the possible systematic faults which could be inserted into what otherwise would be independent pieces of equipment by a single human error. I consider the identification of all significant errors of this type to be totally beyond the grasp of this methodology.

As a final limitation we must consider the concept of design deficiency. The systems analyst assumes, in general, that if all components within a system operate within specified limits the system will perform as intended. Testing is performed to validate the adequacy of this assumption. However, subtle inadequacies may be present which appear when a particular environment is experienced, or the system design requirement may not adequately anticipate necessary factors. Modern technology has produced sufficient examples of these defects so that they should not be considered inconceivable in light water reactors.

It should be noted that the Rasmussen team was cognizant of this limitation to risk analysis as it related to the emergency core cooling system, and parametrically analyzed the sensitivity of plant risk to its operability. The methodology is clearly capable of this type of parametric analysis, and this is, in fact, one of the greatest benefits to be derived from it. However, the potential contribution to risk arising from deficiencies in design is an unquan-

tifiable factor that can significantly increase the actual risk.

The total effect on risk from the factors just discussed is impossible to accurately predict; it would need to be determined by comparison of predicted versus achieved risk levels. However, making such a comparison with statistically significant data is currently impossible due to the small probability levels predicted for significant accidents. Past experience would indicate that the analysis was performed very well if the risk uncertainty band was limited to only a couple of orders of magnitude.

In conclusion, I would like to emphasize the fact that the probabilistic portion of the Rasmussen report represents a significant improvement in the safety analysis of light water reactors, and should be

very valuable in reducing risks. However, the study was performed very much after the optimum time in the design cycle; consequently, it will not have the impact on current reactors that it should have had.

The results presented in the Rasmussen report were misused to the extent of making them appear comparable to actuarial data. The previous aerospace studies did not view similar results in that context. And there exist sufficient methodology and application limitations in this report to make them incomparable in this case as well. It will be sad indeed if the significance of the Rasmussen study should become identified with the accuracy of the point estimates presented in comparison curves such as in their Figure 1.

Probabilistic risk analysis has now been shown to be sufficiently flexi-

ble to be used for weapon systems, space power systems and light water reactors. This proven flexibility bodes well for the suitability of its application to other systems in the nuclear fuel chain. The delay in building fuel reprocessing plants should provide an especially good opportunity for timely inclusion of probabilistic safety analysis as a design tool. It is my hope that sufficient confidence has been gained in the value of probabilistic safety analysis that its application will be required in all future reactor development efforts.

Notes

1. D. Haas, "Advanced Concepts in Fault Tree Analysis," paper presented at System Safety Symposium, June 8-9, 1965, Boeing Airplane Company; and McDonnell Douglas Corp., "Preliminary Safety Analysis Report," MDC-G0744, vols. 3-4 (Jan. 1971).
2. W. F. Vesely and R. E. Narum, "PREP and KIT Computer Codes for the Automatic Evaluation of a Fault Tree," IN-1349, Idaho Nuclear Corp.
3. D. J. Davis, "An Analysis of Some Failure Data," *Journal of American Statistical Association*, 47:258 (1952), 113-150.

The future of reactor safety research

Herbert J. C. Kouts

A number of factors have contributed to a level of public uncertainty about nuclear power that exceeds the reaction usually met by a new technology. One is the apparent suddenness with which nuclear power has come, not allowing much time for people to become accustomed to its existence. A second factor has been the difficulty of understanding it. Nuclear fission seems just too technically remote a concept, unfamiliar to people who feel more comfortable with ordinary chemical combustion because they know it so well. A third reason is the

Emergency core cooling systems are the key; we must improve our estimates of safety margins while avoiding the penalties from unnecessary conservatism

relationship of nuclear fission with nuclear weapons, which leads people to associate the consequences of nuclear weapons with reactor accidents. In addition, estimates of the consequences of worst conceivable accidents at nuclear power plants have generated concern. It is my view that this concern has generally been caused by misunderstanding of the analysis and its meaning. The concern has also been a result of unfamiliarity with risk-benefit methodology.

The views on what measures should be taken to provide protection to the public have changed with time. A few years ago the reactors being built were considerably smaller than now. Attention was then given to providing assurance of safety even if the reactor core were to melt entirely and thus release large

amounts of radioactivity. Such an event could result from an uncontrolled loss of coolant accident. The principal safeguard in this context was the containment building, which was therefore designed to have a low rate of leakage. Even if very large amounts of radioactivity were to be released from the reactor core, it would then be kept well enough away from the public to limit public consequences to reasonably low levels.

Additional protective measures called engineered safeguards were also developed and designed into the power plants. These were generally of two kinds: devices to trap fission products that might be liberated, and devices to control the pressure in the containment building after a hypothetical accident. As long as the integrity of the containment building could be assured, these measures seemed suitably protective.

The increases in reactor size about 10 years ago led to uncertainty as to the ability to ensure containment integrity if the reactor core

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were to melt. For this reason, reliance came to be placed on measures to keep the core adequately cooled even if a loss of coolant were to occur. The emergency core cooling systems (ECCS) that were previously included in designs for supplemental safety were suddenly regarded as necessary. So the emphasis in safety research programs was shifted from control of the release of radioactivity from a molten core to improved understanding of the effectiveness of the emergency core cooling systems, needed to guarantee effectiveness.

The present reactor safety research program concentrates heavily on development of an improved engineering basis for the analysis of effectiveness of the emergency core cooling systems of reactors. We are now in fact well along in a vigorous but lengthy program to establish this improved engineering basis. The future program in reactor safety research is therefore largely the future of this ECCS program.

Before this future program is described, however, and its strong and weak points discussed, it is important to address the question of present adequacy of the assurance of the safety of water reactors. It may well be asked, if the reactors are already safe, why are we doing all this research on safety?

First, how safe are the reactors? I think they are probably much safer than the analysis done for licensing purposes presently indicates them to be. This is partly because we are confident that the methods sanctioned by the Nuclear Regulatory Commission (NRC) for analysis of the effectiveness of the emergency core cooling system have built-in conservatisms that lead to underestimating the safety margins.

Some of these conservatisms have been included with the intent of staying on the safe side where the basic engineering information is not as full or reliable as desired. Some are included because we have not finished our full test program, and we wish to have reserve margins of safety in case we are unpleasantly surprised by something unforeseen. We would like to arrive at a better and more solidly founded estimate

of the safety margins for the improved level of assurance this would give us. At the same time, we are very aware that unessential conservatism can lead to unnecessary financial burdens placed on the public through electric bills that are higher than they need to be. We need the research to improve our position between too much conservatism and not enough.

There is no substantial body of opinion that I know of supporting a view that the present methods used by the NRC to assess reactor safety are unsafe or not conservative. On the other hand, there is a very gener-

We are planning a number of future programs related to core melt sequences, but we should not try to make LOFT part of this program. The results might be interesting, but they would not be enlightening.

al consensus that the case for adequacy of these methods needs improvement. We would all like to be more sure of reactor safety than we now are. This is also how I interpret the APS study: that is, they have not uncovered reasons for substantial short-range concern regarding risk of accidents in light water reactors; they do discuss how the research program should be improved.

The present reactor safety research program of the NRC follows classic lines. Fundamental engineering studies are made where the data base or the general level of understanding is not as reliable or complete or unambiguous as desired. These fundamental studies include work to improve knowledge of material properties and basic processes. The data are then incorporated into mathematical models that describe the processes important in analyzing the course or consequences of hypothetical accidents. The mathematical models are then synthesized into full computer codes used to analyze the full sequence of events during hypothetical accidents. Engi-

neering tests of varying levels of size, realism, and completeness are performed to test the predictive power of the codes.

Clearly this process can be iterated, with fuller information being generated on a broader class of phenomena, more complex models being developed, and better engineering scale tests being run. We must look ahead to determine the time when the process should be ended because we have arrived at the better level of understanding that is sought. This is how all engineering methods are developed and tested.

Code Development

The objective of the research program is the set of computer codes used to analyze the safety of reactors. The basic experimental program is conducted to improve the data base and the models used in the codes, and the large-scale tests are devised to test the codes. All ECCS codes presently developed and used under the NRC program contain conservatisms. The most well-developed ECCS codes are the 'evaluation model' codes that follow the rules laid down in the AEC's opinion (published December 1974) that provided the criteria to be used in assessing the effectiveness of the emergency core cooling system.

Although there has been considerable discussion of so-called 'realistic' or 'best estimate' codes, and some forms of such codes exist, there really are no NRC codes so far that fully deserve this name. Yet these best estimate codes are badly needed. They are necessary for estimating the conservatism in the evaluation model codes. They are also necessary for studies of the sensitivity of predictions to uncertainties in basic data and for realistic prediction of the course of events during the engineering tests. One of the more important objectives of the code development program at present is the construction of best estimate codes to be used in the ECCS program. The APS study recognized the fundamental need for best estimate codes and recommended greater emphasis on providing them. We must press forward with this program.

It has been pointed out a number of times that the current ECCS codes are one-dimensional, though some of the phenomena that must be analyzed are three-dimensional. The thermal-hydraulics in two regions of a pressurized water reactor (PWR) during a hypothetical loss of coolant accident (LOCA) are especially in need of full three-dimensional description: the core region itself and the 'downcomer,' where the countercurrent interaction between 'blowdown' and ECCS water injection is found. The lack of adequate three-dimensional analysis is compensated by conservatism presently included in evaluation model codes. For instance, the ECCS bypass assumption prohibits any cooling effectiveness of ECCS water until the blowdown has ended.

LOFT Experiment

The APS study stated that the forthcoming loss of fluid test (LOFT) program (power level 55 megawatts thermal) may be too small to be dependable as a test of the codes used to assess ECCS systems for the larger pressurized water reactors. They suggested that larger scale tests may be needed in the future, perhaps approaching full size. They suggested that conclusions one way or the other about the need for such tests may not be possible for several years, and that it may be advisable to begin planning larger tests now in case they are needed later.

The factor of approximately 60 in power level between the LOFT reactor and a larger pressurized water reactor is not really indicative of the scale-up from one to the other. A basic objective of the design of the LOFT experiment has been to achieve performance such that the major features of the LOCA sequence would be faithfully reproduced for purposes of testing ECCS codes. If this objective is achieved, all parts of the code can be tested in a satisfactory way in all their complexity and interaction.

It is not in fact possible to scale the loss of fluid test reactor to duplicate all the features of a large pressurized water reactor that would influence the course of a loss of coolant accident. The radius of the LOFT core is much smaller than that of a large pressurized water reactor, and to keep the overall core shape in some reasonable proportion, it has also been necessary to reduce the core height from 12 feet to 5.5 feet. The downcomer of a LOFT reactor vessel is thinner, and the height-to-diameter ratio of the vessel is greater. Piping is smaller in size. The surface-to-volume ratio of the LOFT vessel is greater, and this difference

will enhance certain effects of heat transfer from walls that would be less important in a large pressurized water reactor. There are other such differences.

Still, calculations that have been made to explore the similarity of loss of coolant accidents have indicated that the LOFT experiment will provide a reasonably good simulation of a loss of coolant accident in a large pressurized water reactor.

We are reviewing the need to start design of a larger test on the chance that such a test may be needed. We shall intensively explore every other means of achieving the desired level of assurance for ECCS analysis, be-

NRC safety research which may be expanded

The following are some principal areas of research now underway that the Nuclear Regulatory Commission is considering for expansion, in light of the recommendations made by the American Physical Society's study.

Three-Dimensional Problems. The one-dimensional description of such regions as the core and the downcomer is not satisfactory, particularly because it is not compatible with the need for realistic best estimate codes. One obstacle to improvement at present is that we do not yet have the results of certain experiments needed to guide and test these complex codes.

The plenum fill experiment (PFE) now being constructed at Hanford (Wash.) will supply a better experimental basis for downcomer analysis, and will even provide tests of downcomer two-phase countercurrent mixing and flow at nearly full geometric scale. We are now considering how to accelerate the schedule of the plenum fill experiment.

Experimental work on the three-dimensional flow distribution in the forest of fuel rods comprising a PWR reactor core will be more difficult to conduct in a meaningful way. Some three-dimensional flow effects during reflood were studied during the full length emergency cooling heat transfer (FLECHT) tests of a pressurized water reactor, but these tests were confined to a rod region the size of a single fuel bundle. Some features of three-dimensional flow during blowdown will be studied in the blowdown-heat-transfer experiment just beginning at the Hollifield National Laboratory (Tenn.).

The open-lattice character of pressurized water reactor cores suggests strongly that determining the full effects of flow distribution will require larger bundles. We are now reviewing the possibility of experiments with large electrically-heated bundles of rods, for more realistic study of three-dimensional flow. Such experiments will be complex and expensive, but they may be necessary if we are to develop satisfactory understanding of three-dimensional effects in PWR cores during hypothetical accident conditions.

With the corrections and improvements now underway, the Rasmussen report should be perfectly adequate for use in judging the risk of nuclear power.

cause a large test would be very expensive.

On the other hand, I do not agree with the suggestions which are sometimes made that the LOFT program should later be turned into a study of the consequences of a core melt. The LOFT facility and its containment building were not designed for this purpose. The consequences of an uncontrolled loss of coolant accident to the LOFT reactor would not be typical of a large reactor after a core melt, and I strongly doubt that any reasonable modification would remove the differences. We are planning a number of future programs related to core melt sequen-

es, but we should not try to make LOFT program part of this program. The results might be interesting, but they would not be enlightening.

So the future of that part of the water reactor safety program directed toward improving the basis for ECCS analysis of pressurized water reactors is likely to follow the directions of present lines, with emphasis increased in some areas to provide greater speed or improved results. New programs will probably be added in some areas, such as large bundle tests of three-dimensional effects, some new code development, and new studies related to the results of core melt. We shall also explore

the need for design of a larger system test than the loss of fluid test.

We also plan to conduct tests to ensure NRC capability to analyze alternate means of injection of emergency core coolant if this should be proposed. The AEC over a year ago stated that other emergency core cooling systems that would avoid steam binding would be preferable. We shall include in the semi-scale experiment and LOFT program tests where ECCS injection is made in the lower and upper plenums and in the core region, with high driving force. These experimental results can then be used to test codes for calculation of effects of steam-water

One further line of development is also needed to produce computer codes incorporating an adequate description of the multi-dimensional aspects of ECCS performance. This is improvement of the structures of codes themselves. The existing codes are homogeneous, which means that they assume that steam and water are uniformly mixed and move together where they are present together. We know that this assumption is unrealistic.

New models are being developed, which include provision for lack of thermodynamic equilibrium between phases. These models provide for mass transfer and heat transfer between phases. Some new codes permit steam and water to move with different velocities and possibly in even different directions. Some codes under development include both kinds of non-equilibrium: mechanical and thermodynamic. Development of advanced codes must be even more strongly emphasized if we are to develop realistic best estimate codes on a timely basis.

Pump Behavior. The behavior of the main coolant pumps of a pressurized water reactor could have an important effect on the consequences of a loss-of-coolant accident. If the pump in the broken loop were to

continue to operate during blowdown following a cold leg break, it would speed up the blowdown. If it continued to operate during reflood, it would reduce steam binding. If the rotor were to lock in a fixed position during blowdown, it would slow the blowdown. If the locking occurred during reflood, it would increase steam binding. Pumps in the intact loops would also affect the flow during successive stages of the hypothetical accident.

The evaluation model calculational models currently include the worst assumption in every case, this being the assumption that leads to the highest calculated fuel clad temperature. There is very little measurement base, however, for the characteristic curves (head vs. discharge rate) of pumps operating on steam-water mixtures.

The Electric Power Research Institute (EPRI) and the Federal Republic of Germany are now jointly sponsoring studies at Combustion Engineering, Inc., to measure the characteristic curves of pumps of several kinds and sizes. The Nuclear Regulatory Commission will follow this program with considerable interest, to determine if further work will be needed. We hope that it will be possible to find fundamental meth-

ods to relate characteristic curves to basic design features.

Fuel Damage. Fundamental studies are also under way in one further area that is of importance for assessing ECCS effectiveness. This area is that of fuel damage mechanisms. A number of kinds of out-of-reactor experiments are being conducted to supplement the in-reactor studies now starting at the power burst facility at the Idaho National Engineering Laboratory. We are confident that the analysis of fuel damage used in analyzing emergency core cooling systems includes a number of conservatisms. One of these is the method used to calculate rate of oxidation of Zircalloy in steam, which is especially conservative at the higher temperatures where the reaction is more rapid. Another is the use of single-rod burst data to estimate clad swelling and flow path blockage.

The program of research in fuel damage mechanisms has been enlarged considerably over the past two years, but it still appears to lag behind other parts of the ECCS program. We are reviewing the balance in this program, and are considering how we can speed up in-reactor testing. The use of other reactors in addition to the power burst facility is being considered.—H.J.C. Kouts

mixing in such circumstances. We will not design new ECCS systems, especially since the Conference Report on the Energy Reorganization Act creating the Nuclear Regulatory Commission expressly said that NRC research should not follow such lines. We will leave this kind of design and development to the Energy Research and Development Administration, the reactor designers, and others.

The ECCS program for boiling water reactors (BWRs) will continue to be smaller. Most of the basic engineering studies in the water reactor safety research programs are applicable both to pressurized water reactors and boiling water reactors. Full system testing of codes is not as vital for boiling water reactors, partly because questions related to steam binding do not arise. We shall add some system testing to current programs, but so far we believe that electrically heated tests will be adequate in this respect.

Other Reactor Programs

The water reactor safety research program of the Nuclear Regulatory Commission is broader in objective than just the solution of ECCS questions. A substantial program is being conducted on primary system integrity, both of pressure vessels and piping. The APS study expressed reasonable satisfaction with respect to assurance of primary system integrity as long as this subject continues to receive attention.

I believe there are some areas still to be explored further, and work in these areas will be continued. The most important are: improved data on fatigue crack growth, crack growth in the elastic-plastic regime, the effects of radiation and environment on crack growth and strength and means of limiting such effects, features of the heat-affected zones near welds in thick carbon steel vessels, crack growth in stainless steel piping of boiling water reactors, crack growth as it might be induced by high thermal stress, and on-line monitoring for crack growth.

We also have a substantial program to improve understanding of earthquakes, tornadoes, floods, etc., as they affect siting and design of

nuclear power plants. The seismicity program is being extended to provide a new review of questions related to the possibility of large earthquakes in the eastern United States. We hope that work under way will help in predictability and assessment of possible consequences of tornadoes, through assembly and analyses of the historical record.

Other NRC research programs are being conducted on the safety of gas-cooled reactors and fast reactors. The magnitude and directions of future programs in these areas will depend to some extent on the pro-

An international attack on the problems of reactor safety can only be beneficial to all because improvement of reactor safety anywhere must be regarded as helpful everywhere.

posals received by the Nuclear Regulatory Commission for licensing such reactors. I do not at this time see any need for large research facilities for the gas-cooled reactor program. But there is a growing belief that the fast reactor program will require large tests of the course and consequences of hypothetical core disruptive accidents.

Such tests are not yet scheduled, but they have been introduced into both ERDA and NRC planning.

In fact, it is likely that at least one and probably several large test facilities will be needed to provide better information on fuel-coolant interactions, the conversion of thermal energy to mechanical work in such an interaction, and basic information related to the possibility and consequences of recriticality by recombination following a disassembly. Other work will be necessary to guide studies of effectiveness of core catchers if these should be considered necessary. Large fast reactor facilities will be so costly that it will be necessary to explore how they

might be shared by ERDA and NRC without compromising the independence required by the NRC's research programs.

Risk-Benefit Analysis

The Reactor Safety Study (WASH-1400) was performed for use in judgments on the societal risk of nuclear power. For this application, an accuracy of one to two orders of magnitude should be sufficient. When the study is issued with the corrections and improvements now underway, it should already be perfectly adequate for this application. There will be strong pressure to extend the methodology to assist in improving design, reviewing the adequacy of design of specific plants, etc. I am convinced that these methods will be used more and more as time passes, and that their acceptance will continue to grow. But the extension to such other uses should be made with some care and thought, recognizing the limits of uncertainty attached to the estimates.

International Participation

As a result of the Administration's Project Independence, a strong interest has developed in broadening the safety research program to include participation by other interested countries. A series of international meetings have been held by the International Energy Agency in which these possibilities have been developed. A number of NRC's research programs have now been opened to participation by other countries, including the loss of fluid test (LOFT), the power burst facility (PBF), the plenum fill experiment (PFE), the semiscale experiment, and the heavy section steel technology (HSST) programs.

Other countries with research programs are also proposing that some of these be opened to international participation. Most of the arrangements entered into by the Nuclear Regulatory Commission will be on a quid-pro-quo basis, with an exchange agreement being worked out. In addition, an agreement has just been concluded with the Federal Republic of Germany for their participation in the LOFT program in

return for \$1 million a year for four years. A similar agreement is being negotiated with Japan, and a number of other countries have expressed interest in joining the LOFT program for financial considerations.

I find this trend to be encouraging and exciting. An international attack on the problems of reactor safety can only be beneficial to all, because improvement of reactor safety anywhere must be regarded as helpful everywhere.

It is also widely recognized that there is a need for a more acceptable

basis for scientific confidence in the safety of the nuclear power plants on which, it seems, we must all increasingly depend. Better understanding is necessary so that we can improve our estimates of safety margins, and so we can avoid the penalties from unnecessary conservatism. The future program in water reactor safety research will be pursued with this objective. Objectives have been identified for programs related to other advanced reactors, and these will guide research in these areas. Risk-benefit analysis will become

increasingly important and more widely used. The reactor safety research program is becoming international in scope. Thus the prospect for reactor safety research is for wider and more intensive programs over the next few years.

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A perspective on the debate

Frank von Hippel

One of the major findings of the American Physical Society's reactor safety study was that a nuclear reactor core meltdown accident followed by a major release of gaseous and volatile fission products to the atmosphere (that is, a containment failure) could well have exceedingly severe consequences. In the past this would not have been surprising; that is, before the Atomic Energy Commission's Reactor Safety Study (the Rasmussen report), it was assumed that this would be the case.

The Rasmussen report challenged that idea—that a nuclear reactor core meltdown accident with containment failure could have such severe consequences. The APS study group reviewed the calculations made in that report and found, using essentially the same assumptions, that two omissions in the calculations made in the AEC study had resulted in an underestimate of the average number of deaths from their 'reference' accident by one or two orders of magnitude (see figure). Other major underestimates were

Fission, oil and coal power all have the potential for triggering major social or environmental instabilities; effective citizen watchdogs and a much reduced energy growth rate are required if these dangers are to be avoided

also identified in the Rasmussen report's calculations of other categories of consequences, including genetic defects, nonfatal cancers, and groundwater contamination.

Thus, it appears that the APS study may have the dubious honor of restoring the status quo: a reactor meltdown accident with containment failure could indeed be very serious. Whereas the AEC's Rasmussen report may have unintentionally conveyed the message that we can relax about nuclear reactor safety, I believe the correct message should be that the industry and its watchdogs have to work as hard as ever at making nuclear reactors safe, and that major improvements are called for in several important areas.

While one can and should be

critical of some of the calculations of the Rasmussen report, I believe that it has also done a great service by making a first attempt to put the reactor safety debate into perspective. In fact, I would like to offer here a perspective which was inspired in part by the approach taken in the Rasmussen report.

The first basic premise is that our society obtains great benefits from electric energy. The second is that none of our current sources of electric energy are risk free. Therefore, the risks associated with nuclear-electric energy must be weighed against those of the alternatives.

In the past, the emphasis in the environmental debate relating to electrical energy focused on the routine impacts of the coal and nuclear industries. This is still true in the case of coal where the emphasis is on bad strip mining practices, the occupational hazards of underground coal mining, and the health

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consequences of the sulfur dioxide emissions from coal burning plants.

In the case of nuclear power, the emphasis was on the small amount of radioactivity released routinely from nuclear power plants. The limits on the allowable releases of radioactivity from nuclear power plants have now been reduced to the point where few critics think that the issue is worth their time any more. The focus of the debate over nuclear reactors, therefore, has shifted to low probability high consequence events. In particular the debate has focused on nuclear reactor core meltdown events—either accidental or as a result of sabotage.

The general impression seems to be that such potentially catastrophic events have no counterpart in our coal or oil fired energy generating technology. I think they do. Let me focus specifically on two: war and climatic change.

War. The United States has been involved in three major wars in the past three decades. The consequences of each of these wars was much worse than any nuclear reactor accident could be. Today one of the major sources of world tension stems from the international oil economy. An important fraction of this oil is used for electrical power generation. It might be fair, therefore, to attribute some significant share of the probability of the next major war to the use of oil for the generation of electricity.

Climate Change. In the case of coal we have an ample domestic resource base, but we also have the potential to cause substantial changes in the Earth's climate. The burning of only a few percent of the world's estimated resources of recoverable fossil fuels has already resulted in an increase in the amount of carbon dioxide in the atmosphere by about 10 percent. By the year 2000 it is estimated that man will have increased the carbon dioxide level in the atmosphere by another 10 to 20 percent. What will the impact be on the world's climate? The climatologists can't tell us because of large feedback effects and natural variations which are not well understood. They do tell us, however, that changes with a signifi-

cant adverse impact on world agriculture *could* occur.*

The continued use of oil and coal therefore also involves—with unknown probability—events with potentially very large consequences. In fact, I think that even a major nuclear accident pales somewhat in comparison with the nightmares of war and famine which I have just conjured up.

The other comparison which may help put the nuclear accident in perspective concerns the cancers and genetic defects which would result from high or low doses of radiation. This is an area where the APS study found potentially much larger consequences than the AEC's study. It should be pointed out,

We cannot as a society afford to have the nuclear energy regulators become fat and sleepy as regulators have in other areas.

however, that the number of additional casualties would be diluted in a very large population (spread over thousands of square miles downwind) and would also be spread out in time over a period of decades after the accident. As a result, the added risk to most of the *individuals* in the population downwind would be quite low—less than one percent, except in the case of thyroid damage. An increase in the cancer death rate of a few tenths of a percent and an increase in the incidence of genetic defects by a few hundredths of a percent would hardly be detectable relative to the current rates of occurrence of about 20 percent and 2 percent, respectively.

This would be a rather Strangelovian statement if it weren't for the fact that the current incidence of cancer and genetic defects may well be in large part man-caused. These cate-

gories of consequences of a nuclear accident would therefore not be unique at all, but would be added to a much larger pool of cancers and genetic defects which we are already inflicting upon ourselves with chemicals, medical x-rays, etc.

These arguments are not intended to imply that we should drop the nuclear reactor safety issue. I offer them simply to explain why it is not so obvious to me at this point that we should abandon nuclear energy more rapidly than fossil power—at least on the grounds of nuclear reactor safety.

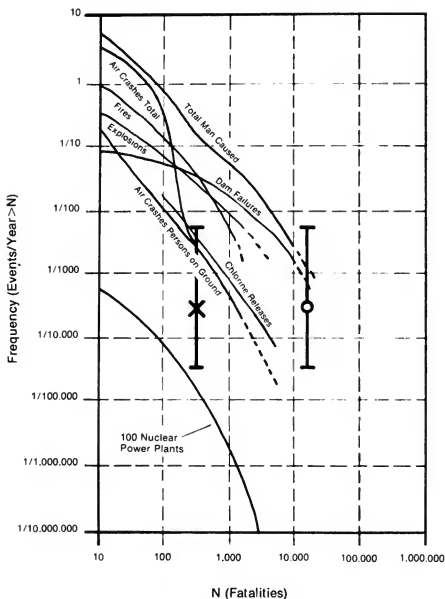
Improving Reactor Safety

In this context then, how might reactor safety be improved? In fact there seem to be some important possibilities. Consider first two of the suggestions made by the APS study: (1) improved containment design and (2) mitigation of accident consequences.

Improved Containment Design. Can the reactor containment building be designed to contain, in fact, a nuclear reactor core meltdown accident? On the one hand there are obvious difficulties. The enormous amount of radioactive energy released by the core after shutdown threatens the integrity of the building in many ways: steam explosions and overpressure, hydrogen and carbon dioxide gas generation and, ultimately, melt-through. On the other hand, the AEC never asked the nuclear power plant designers to design that containment building with the possibility of a core meltdown accident in mind. It would appear to be desirable that this policy be changed.

There are some real opportunities for improvement. In particular, it appears that a containment building could be designed so that the important radioactive isotopes could be trapped in some sort of filter system, even if containment failed because of overpressure. A serious design effort might also produce a 'core-catcher' to prevent melt-through by the core and the associated potential for massive water contamination. And, finally, underground siting would appear to offer protection to the containment building against

*See, for example, J. Murray Mitchell, Jr., "A Reassessment of Atmospheric Pollution as a Cause of Long-Term Changes of Global Temperature," in *The Changing Global Environment*, ed. S. Fred Singer (Dordrecht, Holland: D. Reidel Pub. Co., 1975).



Nuclear and Non-Nuclear Accident Probabilities and Consequences—AEC Estimates and APS Corrections

The curves represent the frequency of different types of accidents, nuclear and non-nuclear, as a function of the number of fatalities as given in the Rasmussen report. The curve labeled "100 Nuclear Power Plants" (50 boiling water and 50 pressurized water reactors) includes only early fatalities, not delayed deaths due to cancer.

Two error bars have been added to this basic figure which appeared as Fig. 6.1 in the Summary volume of the Rasmussen report. The frequency range is that calculated in the Rasmussen report for the occurrence of a "reference accident" assuming the existence of 100 pressurized water reactors. This accident was assigned a probability between 1 in 20,000 and 1 in 2 million per reactor year in the Rasmussen report.

The point 'X' on the left-hand error bar indicates the total number of fatalities, 372 (62 early and 310 delayed from cancer), from the 'reference accident' as calculated in the Rasmussen report. Using the Rasmussen report's probability estimate, but including the corrections to the estimated number of cancer deaths calculated in the APS study, gives the point 'O' or 10,000 to 20,000 cancer deaths.

*A pressurized water reactor core meltdown with a release of radioactivity to the atmosphere almost as great as if there were no containment building at all.

failure, either as a result of a steam explosion inside or as a result of the impact of missiles from the outside—accidental or deliberate.

Having an improved containment structure would probably do far more for reactor safety than adding another safety system onto the reactor itself. As the findings of the Rasmussen report dramatized, new valves and plumbing introduce new accident scenarios as well as preventing the development of old ones.

Mitigating the Consequences. Being in a position to mitigate the consequences of an accident, should one occur, would obviously also be desirable. Currently, the only mitigation strategy is evacuation—and there is a real question whether this could in fact be effective. It might not be possible to evacuate nearby people fast enough; and it would probably not be practical to evacuate the enormous number of people further away who would be exposed individually to relatively low doses.

Mitigating Strategies

Two of the mitigating strategies which the APS study suggested were: (1) the distribution of iodide pills to block the thyroid's uptake of radioiodides, and (2) the development of practicable decontamination techniques to reduce the long-term population doses from the 30-year half-life isotope, cesium-137.

These two efforts might reduce by as much as an order of magnitude the human costs of a major release of radioactivity to the atmosphere. Once again, they could be made much more convincing than improved plumbing for the reactor.

Improved Reactor Design. Let us turn now, however, to the issue of improved reactor design. No group has seriously compared the safety advantages of different reactor designs. It is true that the Rasmussen report did compare the safety of the boiling water and pressurized water reactors and found that these two reactor types were almost exactly equal in safety—a remarkably improbable result incidentally! But no one has done a serious comparison of these reactors with, for example, the much different Canadian heavy

Comparisons are made every day of the economic advantages of different reactor designs; but there seems to be a gentlemen's agreement not to debate the safety advantages.

'No fundamental change in the situation'

Hans A. Bethe

The APS study found that the possible consequences of a severe reactor accident are substantially greater than had been concluded in the Rasmussen report. However, the basic conclusions of the Rasmussen report remain unchanged: The actual risk from reactor accidents is very small indeed.

The APS study did not investigate the probability of reactor accidents nor did it investigate the number of immediate casualties from a hypothetical reactor accident. According to the Rasmussen report, in an average situation of population density and weather, the 'reference accident'—called category PWR-2—will lead to about 300 casualties from radiation, both immediate and delayed. The APS study differs from this estimate of the possible consequences of a severe accident by taking into account certain delayed cancers. They are of two kinds:

- Delayed cancers resulting from high-level exposure of persons close to the reactor accident. These are

mostly lung and thyroid cancers. The APS study calculated 1,100 to 5,600 cancer deaths from high-level exposure. The uncertainty is very large, the geometric mean is 2,500. This number depends on the particular location of the reactor and the weather conditions; and it is 8 times the number given in the Rasmussen report.

- Delayed cancers from low-level radiation which occur at distances between 20 and 500 miles at times later than one day after the accident. These effects are mainly due to cesium-137 which has a half-life of about 30 years and emits gamma rays. The number estimated in the APS study is 10,000, assuming a population density of 300 persons per square mile. The Rasmussen report states that the correct population density to use is not more than 165 per square mile, and perhaps even only half of this number because in many cases points 500 miles from the reactor are in the ocean. The APS number should therefore be multiplied by 0.55, giving 5,500 delayed deaths from cancer.

I consider this number an upper limit. First, because it should be easy to remove the cesium, at least from dwellings, by decontamination. Cesium is about the most reactive element, and forms soluble salts which probably will be washed away by rain even if no active decontamination is undertaken. Second, because most of the bio-medical evidence indicates that the effect of very low-level radiation in producing cancer is much less than proportional to the



total dose received. It is true that the BEIR report recommends using the linear assumption (effect proportional to total dose regardless of dose rate) but the direct evidence, both from animal experiments and from the Hiroshima-Nagasaki experience, strongly suggests that low levels are much less effective.*

Taking nevertheless the calculated number 5,500 we get for the total number of delayed cancer deaths in the 'reference accident' 6,500 to 10,500.

The probability of the 'reference accident' is estimated in the Rasmussen report to be one in 200,000 per reactor year. Assuming 100 reactors of 1,000 megawatts electrical, there is then an average risk of 3 to 5 fatalities per year for the whole of the United States.

It so happens that the probability of the 'reference accident' (category PWR-2) has been greatly decreased

*The Committee on the Biological Effects of Ionizing Radiation (the BEIR Committee) is an advisory committee of the National Academy of Sciences-National Research Council. Its 1972 report on "The Effects on Populations of Exposure to Low Levels of Ionizing Radiation" is commonly referred to as the BEIR report.—Ed.

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water reactor or the high temperature gas-cooled reactor.

Such a comparison might teach us something. Comparisons are made every day of the *economic* advantages of different reactor designs; but there seems to be a gentlemen's agreement not to debate the *safety* advantages. If it ever becomes possible to discuss reactor safety rational-

ly, an open discussion of the relative safety advantages of various design features might lead to some important design improvements.

Making Reactor Designs Foolproof. Another concern which must be addressed is whether, as the nuclear industry grows and as the mystique of nuclear power wears off, the industry will become sloppy. The pioneers who developed this technology had real respect for its dangers. Today, there are many signs that not all those who have inherited the technology have that same respect. This means that reactor technology and regulation will have to be made more protective against fools.

Perhaps, even more importantly, nuclear technology has to be designed better against deliberately destructive acts. When today's technology was being developed, 10 to 15 years ago, no one thought about terrorists. Now we unfortunately have to—and we must, as a result, push our nuclear technology closer to the goal of being intrinsically safe.

The fact that none of the above suggestions were considered seriously by the AEC in the past dramatizes for me the importance of having outside watchdog groups. In fact, it might be appropriate at this point to acknowledge the importance of the efforts of the Union of Concerned Scientists—Henry Kendall in particular—in opening up the whole issue of reactor safety and in attracting the interest of those of us who organized the APS study. I have become convinced by my experiences in this area that the watchdog role must be a continuing one. We cannot as a society afford to have the nuclear energy regulators become fat and sleepy as regulators have in other areas.

The necessary improvements in nuclear reactor safety will not be possible if we as a society go ahead with a crash program to build all of the 1,000 large reactors and associated plants which the government envisions for the next 25 years. Similar concerns can be raised about the consequences of the projected drastic growth in our use of coal. In fact, I believe that every time we as a society double our rate of energy consumption, we decrease our fu-

ture options dramatically and increase the probability of triggering some major social or environmental instability. One might well ponder whether a society that has been unable to implement even the simplest strategies for increasing the efficiency of its energy use can really deal successfully with the more demanding challenges of modern energy technology.

A perspective on nuclear reactor safety would be incomplete if it ignored the safety issues which relate to the rest of the nuclear fuel cycle.

It is at the reprocessing plant, for example, where the zirconium sheath around the fuel will be chopped up and the ceramic fuel pellets will be dissolved, freeing enormous amounts of radioactivity in gaseous and liquid form. This is also where plutonium first becomes available for diversion or theft. When we have a better perspective on nuclear power I believe that we may realize that it is at the reprocessing plant and not at the reactor where the key issues of safety and environmental compatibility of nuclear energy will be found. For this reason I am proposing that these issues be examined in the next outside technical review of fission power.

In summary,

- The consequences of a nuclear reactor accident or sabotage could be much more serious than indicated by the Rasmussen report.

- It does not necessarily follow, however, that the social risks associated with the nuclear reactor accidents are more severe than those associated with fossil fuels.

- But this comparison provides scant comfort and since there are major opportunities for improving reactor safety, they should be pursued.

- Such improvements are unlikely to be implemented without the continued involvement of independent citizen and scientist gadflies and a serious national commitment to a much reduced growth rate in our energy use.

- Finally, much more attention should be devoted to the issues associated with other parts of the nuclear fuel cycle—starting with the nuclear fuel reprocessing plant. □

by a design change recommended as a consequence of the Rasmussen report: The main accident cause in category PWR-2 came from the presumed failure of a check valve. The manufacturer has cured this trouble, and thereby the probability of this accident category has been reduced to about 20 percent of its former value. However, if we include other accident categories and weight them with the amount of radioactivity released in each case, we get for the weighted probability of *all* accidents involving substantial release, again, the figure of one in 200,000. The estimate of 3 to 5 fatalities per year can, therefore, be considered as the total risk from nuclear accidents.

When extrapolating to the future, it should be kept in mind that reactor safety is not a static matter. Safety research will be conducted, in fact with a considerably increased budget, and there is every reason to expect that the safety will be greatly improved over the Rasmussen assumptions by the time 1,000 nuclear reactors are working. The risk at that time might be even lower than 3 to 5 fatalities per year.

Another way of looking at the APS results is given in the APS study itself: The population that is exposed to fallout from the reactor has an average probability of contracting fatal cancer due to the accident of only 0.1 percent. In the United States, this should be compared with a 20 percent probability of contracting cancer from other causes.

Considering all this, I conclude that the APS study has not fundamentally changed the situation. While it is true that a single large accident may lead to several thousand fatalities, most of them delayed and spread over many years, the average risk per year remains at an extremely low level. □

More than a hundred million tons of uranium tailings have already been accumulated in the western United States. At one site in southwestern Colorado, shown below, the two hills behind the town of Durango are in fact 1.6 million tons of tailings piled 230 feet high and spread over 146 acres. The edge of the pile is about 600 feet from Main Street (bottom).



David Dinsmore Comey

**Fatal health effects per
gigawatt-year of
electricity generated:
coal 50, nuclear 400+**

The legacy of uranium tailings

A typical coal-fired power plant probably kills about 50 people per year with its air pollution whereas a typical nuclear power plant of the same capacity probably kills an average of 0.01 people per year. A coal-fired power plant kills more people every few days than a nuclear power plant will in its 30 or so years of existence.—*Bernard L. Cohen*

Last year an article in the *Bulletin* by Bernard L. Cohen opened with the above provocative statements.¹ The health impact of 50 deaths per gigawatt-year from coal-fired plants was almost entirely due to sulfur oxides released from the plant stack.* The health impact of 0.01 deaths per gigawatt-year from a nuclear plant came from radioactive effluents released during normal plant operation, and assumed there would be no deaths from nuclear plant accidents. Other comparisons by nuclear proponents have reached similar conclusions.²

Evidence is now emerging, however, that the health impact from the nuclear fuel cycle has been grossly underestimated. Robert O. Pohl,

professor of physics at Cornell University, has made a major contribution to our knowledge of these effects in an important but as yet unpublished paper. Pohl concluded that due to thorium-230 decay alone, the health impact of nuclear-generated electricity is at least 394 deaths per gigawatt-year.³ Thus, if the number of U.S. nuclear power plants projected for the year 2000 by the former Atomic Energy Commission are actually built, the result will be at least 5,741,500 future deaths from lung cancer over the next 80,000 years. A little over 3 million of these deaths will be among the population of the eastern United States, the remainder in the rest of the Northern Hemisphere.⁴

How could such enormous health effects have been overlooked? Probably because almost everyone has focused on emissions from the nu-

clear power plants and the fuel reprocessing plants, and virtually ignored the other end of the uranium fuel cycle.

Presently mined uranium ore contains 0.1 to 0.2 percent uranium and only 0.71 percent of that uranium is fissionable uranium-235.* Thus more than 100,000 tons of uranium ore must be mined to produce one gigawatt-year of electricity.

At the uranium mills, located in the western United States, the uranium ore is crushed and ground, then the uranium is chemically separated and the residue discarded on a tail-

*It is the fissile uranium-235 which is fabricated into fuel elements for the nuclear reactors used commercially in the United States to generate electricity. After the uranium is mined, milled and enriched, it is fabricated into these fuel elements or rods used in the reactor's core. The many stages of the nuclear fuel cycle are discussed by John Holdren in "Hazards of the Nuclear Fuel Cycle," *Bulletin* (Oct. 1974).

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*One gigawatt-year equals 8,760,000 megawatt-hours (the amount of electricity generated in a year by continuous operation of a million-kilowatt power plant).

ings pile. When the pile dries out, radioactive radon-222 gas routinely escapes into the atmosphere, where it travels long distances.⁵ Radioactive isotopes formed by the decay of radon-222 include the 'radon daughters' polonium-218 and polonium-214. These isotopes of radon have chemical and physical properties that cause them to be deposited in the bronchial epithelium or tissue of the human lung, leading to high radiation dose rates to a specific region of the lung, the bronchus, where lung cancers are most likely to arise.⁶

Thorium-230

The isotope in the uranium mill tailings which is the source of the radon and its lethal daughters is thorium-230, whose radioactive half-life is 80,000 years.⁷ This assures that radon-222 will continue to be produced from a tailings pile for more than a million years, with consequent health effects on human beings.

The U.S. Environmental Protection Agency, in its 1973 report on the uranium fuel cycle, estimated that approximately 5 percent of the

radon generated in a tailings pile 16 feet thick would escape from the pile as a gas. Based on this release rate, the EPA estimated that in the first 100 years, a typical uranium mine tailings pile of 250 acres would result in 200 health effects.⁸ A recent modification by EPA of their air dispersion model has reduced that to approximately 60 health effects during the first 100 years.⁹

Since those 'health effects' are lung cancers, for which even the most medically advanced survival rate is less than 5 percent, Pohl has assumed that 57 lung cancer deaths would result in this 100 year period from each such pile. Because only 0.00091 of the thorium-230 in the pile will decay during this 100 year period, however,¹⁰ the total number of deaths over time will be enormous; and based on the EPA assumption that 159 gigawatt-years of electricity¹¹ will be generated from the uranium-235 that produced this tailings pile, Pohl calculates the fatality rate to be 394 deaths per gigawatt-year.*

$*(57/159)/0.00091 = 394 \text{ deaths per gigawatt-year.}$

If this estimate of 57 deaths per uranium tailings pile per 100 years and the EPA's estimate of 37,620 tons total production of uranium yellowcake per mill are correct,¹² then given the 6,897,000 tons of yellowcake that will be required for the 1,090 gigawatts of reactors the AEC expects will be operating in the United States by the year 2000, I estimate that the total number of deaths resulting from this uranium production will be 5,741,500 over the next 80,000 years.*

Pohl has probably erred on the conservative side in his calculations. For one thing, he uses the 1973 EPA estimate that one such model tailings pile equals 159 gigawatt-years of electricity.¹³ The AEC's 1974 report, WASH-1139(74), indicates that 142.5 gigawatt-years per pile is a more realistic figure.¹⁴ This increases the fatality rate to 440 deaths per gigawatt year.**

Moreover, Pohl's calculations assume no growth in population either in the United States or in the world over the 1970 population. While such a calculation is very valuable in terms of forming a constant baseline from which the health impact from various growth models can then be calculated, in itself it probably is not realistic.

The EPA report assumed that the U.S. population reached 300 million in the year 2020 and then stabilized. The world's population was assumed to increase at a 1.9 percent annual growth rate from a 1970 base of 3,560,000,000.¹⁵ When this EPA growth model is taken into account, the fatality rate rises to 982 deaths

$*(57/0.00091)/37,620 = 1.665 \text{ deaths per ton; } 6,897,000 \text{ tons} \times 1.665 \text{ deaths per ton} = 11,483,000 \text{ deaths; half of these deaths (5,741,500) will occur in the next 80,000 years during the first half-life of the thorium-230.}$

The AEC estimated a cumulative requirement of 2,187,000 tons of yellowcake by the year 2000. At their estimated annual consumption of 157,000 tons per year, 30 years operation would result in a total of 6,897,000 tons: $(157,000 \times 30) + 2,187,000 = 6,897,000$. This is for 1,090 gigawatts of nuclear plants on line by the year 2000. See *Nuclear Power Growth 1974-2000*, WASH-1139(74) (Washington, D.C.: Atomic Energy Commission, Feb. 1974), p. 3, Table 4, Case D. Hereinafter referred to as WASH-1139(74).

$** (57/142.5)/0.00091 = 440 \text{ deaths per gigawatt-year.}$



"And your mortgage payments terminate 2004 or the end of the world, whichever is first."

per gigawatt-year.* This corresponds to 12,825,000 deaths over the next 80,000 years—if AEC predictions of 1,090 gigawatts of nuclear power plant capacity in the United States by the year 2000 are met, and if these plants operate for 30 years.**

Based on the foregoing, it would seem to be a myth that the lethal health effects from coal-generated electricity are 5,000 times greater than the lethal health effects of nuclear-generated electricity as estimated by Cohen and others.¹⁶ The deaths induced by the decay of thorium-230 in uranium mill tailings alone seem to swing the statistics in the reverse direction, and further analysis of other parts of the nuclear fuel cycle may identify additional health effects that have been overlooked.

The Atomic Industrial Forum, the American Nuclear Society and others may argue that very few of the thorium-induced deaths will occur during our lifetimes, and that it is unfair to make such a comparison of current deaths from coal-generated electricity with future deaths from nuclear-generated electricity. But that makes the disparity a moral issue: Do we have the right to con-

*This assumes that the non-U.S. health effects (lung cancers) after the first 100 years result from a stable world population corresponding to the average during the first 100 years. This means 90 health effects outside the United States during the first 100 years, 44 health effects inside, for a resulting figure of 982 deaths per gigawatt-year:

$$((144 + 90)/(142.5/0.00091) \times 0.95 = 982 \text{ deaths per gigawatt-year.}$$

This calculation assumes that all the uranium-235 made into reactor fuel is actually fissioned to make electricity. In reality, however, about one-quarter of it is not fissioned and remains trapped in the spent fuel elements. The AEC seems to assume that all but 1.3 percent of it will be recovered by reprocessing the spent fuel and by shipping the recovered uranium to an enrichment plant where the uranium fuel cycle will start once again (WASH-1139(74), p. 26). So far this has not been the case, however, and the economics of fuel reprocessing make it unlikely in the future. Correcting the above calculations for this factor would raise the fatality rate well over the 1,000 deaths per gigawatt-year level.

**See first note, col. 3, p. 44. $(134 \times 0.95)/(0.00091/37,620 = 3,719 \text{ deaths per ton.}$ Thus 6,897,000 tons = 25,650,000 deaths; half of these deaths (12,825,000) will occur during the first 80,000 year half-life of thorium-230.

sume electricity from nuclear fission plants for the next few decades, forcing thousands of future generations to suffer the lethal consequences?

Theoretically we could have a national referendum and decide that 50 deaths per gigawatt-year from coal-generated electricity was a debt we are willing to pay ourselves. But how can we presume to make such a decision involving the lives of our descendants?

Even if no future generations were involved, there would still be a moral issue: Close to half the thorium-induced deaths will occur outside the United States, but the electricity will be consumed by the U.S. economy. That is likely to be regarded as a paradigm example of imperialism.

What is to be done?

In the case of coal-fired plants, the lethal health effects of the sulfur oxides can be considerably reduced either by removing them from the stack gases after the coal is burned, or through desulfurization.¹⁷

Oak Ridge National Laboratory estimates that 90 percent of the health effects of radon-222 from tailings piles can be eliminated by using a 20-foot earth cover over the pile;¹⁸ but past experience with erosion by wind and flooding makes it clear that this would be an intermediate solution for only a few decades, and even this would cost about a billion dollars.

Removal of long-lived radionuclides such as thorium-230 from the tailings prior to their disposal could be accomplished at considerable economic cost. However, inasmuch as the disposal of high-level radioactive wastes from reactor spent fuel remains unresolved, this additional step would hardly assist in solving that problem.

Mixing thorium-contaminated tailings with cement and pumping the slurry down into abandoned uranium mines may present other long-term problems, not to mention the fact that the volume of the tailings is greater than the volume of the uranium ore removed from the mine, so not all of it would fit.¹⁹

Any solution is probably going to raise the price of uranium substan-

tially, and may result in making nuclear fission plants so uneconomical in comparison with alternate sources of electricity that no further construction of nuclear plants will occur.

This eventuality, however, should not be allowed to result in the neglect of the more than hundred million tons of uranium tailings already accumulated in the western United States.

Notes

1. Bernard L. Cohen, "Perspectives on the Nuclear Debate," *Bulletin*, 30 (Oct. 1974), p. 35.
2. Leonard A. Sagan, "Human Costs of Nuclear Power," *Science*, 177 (1972), p. 487; Richard Wilson, "Kilowatt Deaths," *Physics Today*, 25 (1972), p. 73. See also Lester B. Lave and Linnea C. Freeburg, "Health Effects of Electricity Generation from Coal, Oil and Nuclear Fuel," *Nuclear Safety*, 14 (Sept.-Oct. 1973), p. 473.
3. Robert O. Pohl, "Nuclear Energy: Health Effects of Thorium-230" (Ithaca, N.Y.: Department of Physics, Cornell University, May 1975). Pohl's study was done with the support of a fellowship by John Simon Guggenheim Memorial Foundation.
4. Based on air distribution model used in *Environmental Analysis of the Uranium Fuel Cycle, Part I—Fuel Supply* (Washington, D.C.: U.S. Environmental Protection Agency, Oct. 1973), p. 73, as modified by letter of July 2, 1975 from William H. Elett, EPA's Bioeffects and Analysis Branch to Bernard L. Cohen, University of Pittsburgh. The model and the report will be hereinafter referred to as the '1973 EPA Report.'
5. Elett to Richard Wilson, Harvard University, July 2, 1975. Elett said: "We do not agree that the 1.64 day half-life of radon confines the impact of tailings piles to remote areas. Both our studies and Lesterj Machta's indicate about 50 percent decay between the average tailings piles sites and the eastern U.S. seaboard."
6. EPA Report, Part I, p. 64. Polonium-218 and polonium-214 emit alpha particles with very high energies (6.0 and 7.7 million electron volts respectively). See Merrill Eisenstadt, *Environmental Radioactivity* (New York: Academic Press, 1973), p. 208.
7. Pohl, "Nuclear Energy."
8. EPA Report, Part I, p. 73.
9. EPA Report and Elett to Cohen, July 2, 1975.
10. Pohl, "Nuclear Energy," Fig. 1.
11. EPA Report, Part I, p. 25.
12. EPA Report, Part I, pp. 24-25. "Yellowcake" is U_3O_8 , or uranium oxide, which is ultimately converted into reactor fuel.
13. EPA Report, Part I, p. 25.
14. The average light water reactor requires 264 tons per year of uranium yellowcake to produce 1 gigawatt-year of electricity. See WASH-1139(74), p. 24, Table 12. The EPA's model mill is assumed to produce 37,260 tons over its lifetime: $37,620/264 = 142.5 \text{ gigawatt-years.}$
15. EPA Report, Part I, p. A-26f; Part II, p. D-15.
16. An article by Lave and Freeburg concluded that a pressurized water reactor "appears to offer 18,000 times less health risk than a coal-burning power plant" (*Nuclear Safety*, 14 (Sept.-Oct. 1973), 423). The authors also concluded that "Radiation exposure to the public from the current effluents of uranium mines and mills and plants involved in lead-metals production, isotopic enrichment, and fuel fabrication is not considered significant compared with doses from power-plant or reprocessing-plant effluents. For example, it has been estimated that the total population dose from current uranium-mill effluents per annual fuel requirement produced for a 1,000-megawatt electrical power plant is no more than 0.06 man-rem, primarily from airborne thorium-230."
17. Significant breakthroughs in both technologies have been recently announced by the Illinois Institute of Technology and the Battelle Memorial Institute.
18. EPA Report, Part I, p. 46.
19. Pohl notes the interesting fact that the volume of uranium tailings involved in generating one gigawatt-year of electricity is of the same order of magnitude as the volume of fly ash from a gigawatt-year's worth of coal-generated electricity. This seems to have been overlooked by those environmentalists who argue that the use of uranium to generate electricity has far less impact on the environment than use of coal. At least fly ash can be mixed with concrete for construction purposes: such uses of uranium tailings (for example, Grand Junction, Colorado) are no longer permitted.

Failure at the NPT review conference

Inflexibility of the nuclear nations produces another in the long series of lost opportunities in arms control; the tide of proliferation flows on

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The world has witnessed the first confrontation between the 'nukes' and the 'non-nukes.' Although only a political one, this confrontation at the Review Conference of the Parties to the Non-Proliferation Treaty (NPT), which took place last May in Geneva, portends serious trouble ahead.

The Review Conference was at first hailed as the most important arms control conference since World War II. The participants hoped that the conference would help to strengthen the NPT, whose credibility was badly shaken a year ago by India's explosion of a nuclear device, and help to persuade some of the near-nuclear countries that had never joined the Treaty to sign up and some of the reluctant signatories to ratify. The participants had been encouraged by the ratification of the NPT in April by the Euratom countries (West Germany, Italy, Netherlands, Belgium and Luxembourg) and by South Korea.

Despite the renewed impetus given by these accessions, the conference was a dismal disappointment and ended without any tangible results. Only 57 of the 96 parties and only one Foreign Minister, Canada's Allan MacEachan, considered the conference important enough to merit their participation. In addition, one of the parties to the Treaty (Iraq) sent an observer. Of the 16 countries that had signed but not ratified the Treaty, 7 were present—Egypt, Japan, Panama, Switzerland, Trinidad and Tobago, Turkey, and Venezuela—but not Indonesia, a potential nuclear power. Of the 38 countries of the world that had not signed the Treaty, 7 attended as observers—Algeria, Argentina, Brazil, Cuba, Israel, South Africa and Spain. All of these seven non-signatory nations are near-nukes or potential nukes.

The conference largely repeated the pattern of recent international conferences on such global problems as development, environment, population, food, law of the sea, energy and raw materials—namely, a confrontation between the 'have' and the 'have-not' countries. But unlike those other conferences, where the parties were trying to grapple with new problems or new

aspects of old problems, at the NPT Review Conference the non-nukes were asking only that the nuclear powers live up to treaty commitments already undertaken by them.

The Third World and non-aligned non-nuclear countries made a number of specific demands from the nuclear powers. The following demands were the most important:

- an end to underground nuclear tests,
- a substantial reduction in nuclear arsenals,
- a pledge not to use or threaten to use nuclear weapons against non-nuclear parties to the Treaty,
- substantial aid to the developing countries in the peaceful uses of nuclear energy,
- the creation of a special international regime for conducting peaceful nuclear explosions (PNEs),
- equality of treatment as regards safeguards, and
- agreement to respect all nuclear-free zones.

The nuclear powers were inflexible and adamant. They rejected all those proposals out of hand. The two superpowers considered the negotiations in the Strategic Arms Limitation Talks (SALT) as their exclusive preserve and the Soviet Union even warned the non-nuclear powers not to meddle in these negotiations. They pointed with pride to the SALT agreements, the threshold test ban treaty and the Vladivostok agreement, although most of the non-nukes regard these agreements merely as blueprints for the continued vertical proliferation of nuclear weapons under agreed terms and conditions. They even praised the U.N. Security Council resolution promising positive security assurances to the non-nukes, although the latter tend to consider it worthless. The nukes were interested only in technical questions—tightening and strengthening international controls and safeguards on exports and on the peaceful activities of the non-nuclear powers, while refusing to accept them fully for themselves. This was something which the non-nukes regarded not merely as discriminatory but as a form of nuclear neo-colonialism.

While the nuclear powers and the

major supplier countries wanted more effective safeguards against possible diversion of fissionable material, the safeguards they themselves proposed on exports did not go nearly far enough. Unless safeguards apply to the entire fuel cycle they leave a loophole large enough to drive a nuclear bomb through. Safeguards on *all* nuclear activities are applied to Treaty parties but *not* to non-parties, who need them most. The nuclear powers and the other suppliers of nuclear reactors and material refused to go that far because of shortsighted commercial competition. This absurd discrimination which places non-parties in a more advantageous position than parties has little legal and less moral basis. It is politically and technically unwise and is likely to facilitate rather than prevent the further horizontal proliferation of nuclear weapons to additional countries.

The nuclear powers supported the idea of having the International Atomic Energy Agency (IAEA) elaborate further recommendations for the physical security of nuclear materials in order to minimize the danger of their theft or hijacking. They also favored the establishment of regional or multinational nuclear fuel cycle centers which would reduce the possibilities of theft and diversion of fissionable material. But the nuclear powers had only the vaguest ideas as to how the non-nuclear countries, particularly those that already had pilot facilities and/or wanted their own large-scale plants, might be persuaded to agree to regional instead of national fuel enrichment, fabrication and reprocessing centers. They also had no plans or proposals as to who would pay for the heavy costs of financing and constructing such centers. They were willing to have the IAEA study the matter but made no commitments for the practical implementation of such projects. Thus, although these ideas have merit and were broadly accepted, they did not generate much enthusiasm.

The non-nuclear parties might have been willing to accept such restraints and controls if the nuclear powers had been willing to accept any concrete or binding limitations

on their own freedom of action in the nuclear field. The nukes, however, were prepared to agree only to a cosmetic and tranquilizing declaration in which they would merely repeat their unfulfilled treaty pledges and promise to try a little harder. This, of course, was unacceptable to the non-nukes who felt that they had been deceived by the nukes ever since the Treaty was signed in 1968. The Third World or "Group of 77" countries, led by Mexico's Ambassador Alfonso Garcia Robles, the dominant figure at the conference, revolted against the hegemony of the superpowers. No group could command the required two-thirds majority and a deadlock ensued. But even large majority votes, or indeed anything short of a genuine consensus, at a conference such as this would have been an indication of failure.

Actually, if the nuclear powers and other supplier states are really serious about improving safeguards and controls on nuclear materials, they could do so by agreement among themselves either through the IAEA or outside of it, without the backing or support of the NPT Review Conference. The conference merely provided an opportunity for

tors but also the facilities and technology for the complete nuclear fuel cycle, including uranium enrichment and the plutonium reprocessing facilities. While all these plants and nuclear materials are to be under IAEA safeguards, and Brazil has undertaken not to produce any nuclear explosives, there is little or no recourse if Brazil should decide either to abrogate the agreement, or to build its own plants to use its indigenous uranium resources.

On the subject of peaceful nuclear explosions, the United States had doubts about the feasibility and wisdom of conducting such explosions, but the Soviet Union considered that their feasibility for industrial applications had already been established. The conference noted that the potential benefits of PNEs could also be made available to non-nuclear states that were not parties to the NPT, under conditions that would not lead to the proliferation of nuclear explosive capability, and that the entire question required further study. The nuclear powers and their allies considered that the IAEA should play the central role in this whole field and should set up machinery for intergovernmental discussions. The Third World coun-

The nuclear powers were prepared to agree only to a cosmetic and tranquilizing declaration in which they would merely repeat their unfulfilled treaty pledges and promise to try a little harder.

focusing attention on the problems. But it seems that the supplier states are more interested in sales and profits and are unwilling or unable to take the necessary decisions.

The ineffectiveness of the 'suppliers club' and the difficulties facing the proposals for regional or multinational nuclear fuel cycle centers was demonstrated by the West German agreement to sell to Brazil not only several nuclear reac-

tors, who fear that the IAEA is dominated by the nuclear powers, wanted a formal meeting of the parties to the NPT to conclude the special international agreement on PNEs called for by the NPT. The nuclear powers, however, were opposed, and agreement on this matter was not possible.

It is almost incredible that the nuclear powers—whose interest in the survival of the NPT is far greater

than that of the non-nuclear powers—who, unlike the non-nukes, have not lived up to their Treaty obligations could have been so insensitive to the legitimate and not unreasonable demands of the non-nuclear powers. Some of the non-nuclear powers felt that only a combination of arrogance and immorality could lead the nukes to insist that the non-nukes forswear the acquisition of nuclear weapons while they continued to add to their fearful nuclear over-kill capacity and, at the same time, to refuse to promise that they would not use or threaten to use these weapons against non-nuclear parties to the Treaty. Ironically, only China and France, which are not parties to the NPT or the U.N. Security Council resolution on security assurances, are prepared to give such 'negative security assurances' to all non-nuclear states.

The non-nukes were also annoyed that the Soviet Union conducted an underground nuclear test on the eve of the Review Conference and that the United States conducted one in the middle of the Conference. Some of the non-nukes speculated in private as to whether the timing of these tests was coincidental or deliberate.

Although it was certainly not their intention, the actions of the nuclear superpowers seemed almost calculated to anger the developing Third World countries and do damage to the Treaty. The NATO and Warsaw Pact allies of the United States and the Soviet Union—all of whom are under the nuclear umbrella of one or the other superpower and nearly all of whom are rich developed countries—gave their support to the superpowers. They were somewhat embarrassed by their leaders but apparently not embarrassed enough. Only Rumania showed unique courage in defecting from the Soviet position and by joining the ranks of the rebellious Third World countries. Some American allies such as Australia, Iran, Japan and New Zealand also urged the nuclear powers to provide negative security assurances, but without avail.

In the end, in an attempt to save the Review Conference from complete failure, the President, Inga Thorsson of Sweden, proposed her

own draft declaration. It was a bland compromise urging greater efforts by the nukes to carry out all their Treaty commitments. Although it was too little for the non-nukes and too much for the nukes, it provided the basis for an unenthusiastic consensus. Actually the consensus was artificial, indeed non-existent. The "Group of 77" countries, who constituted more than half the participants at the conference, explicitly stated that they would not oppose the declaration because of their regard for Mrs. Thorsson; but that they would continue to insist on their three proposed Protocols to the NPT providing for the cessation of nuclear testing, a drastic reduction in nuclear arsenals and the provision of negative security assurances to the non-nukes by the nukes. Other countries also had official reservations. Yugoslavia announced that in view of the unwillingness of the nukes to carry out their treaty commitments, it would have to 're-examine' its attitude to the NPT.

The superpowers do not yet seem to understand that they cannot impose their will on the non-nuclear powers and that they need them to make the NPT work.

Thus, the declaration did not even succeed in papering over the differences between the two groups.

It is doubtful whether the final declaration will do anything to help the NPT survive. It contains nothing that could provide any incentive to the near-nuclear holdouts to give up their option to go nuclear. On the contrary, it can provide arguments and pretexts for them to go nuclear. Many of the non-nuke parties are angry and it must be remembered that they can withdraw from the NPT on three months' notice. The superpowers do not yet seem to understand that they cannot impose their will on the non-nuclear powers and that they need them to make the NPT work. Nor do they seem to recognize that they have the chief responsibility as well as the major interest in preventing the erosion of

the NPT. It is unlikely that the conference will have shocked the nukes sufficiently to make them reassess their positions and adopt the policies necessary to save the Treaty from oblivion. Unfortunately, the Review Conference turned out to be merely another in the long series of lost opportunities in the field of arms control.

Technical controls and safeguards on the supply and use of all fissionable material, nuclear reactors and plutonium reprocessing plants are obviously useful and necessary. In addition to these 'technical' measures, however, it is imperative that the nuclear powers that are parties to the NPT also undertake the necessary 'political' measures:

- an end to all nuclear weapon tests,
- a real reduction in the size of their nuclear arsenals,
- a pledge not to use or threaten to use nuclear weapons against non-nuclear parties,
- preferential and substantial aid in the peaceful uses of nuclear energy to non-nuclear parties, and
- the creation of a special regime for conducting peaceful nuclear explosions.

One thing is certain. Unless the nukes take effective steps such as these to stop the vertical as well as the horizontal proliferation of nuclear weapons, the world will inevitably be faced with the much more fearsome and much less manageable threat of non-governmental proliferation—the hijacking or acquisition of nuclear weapons by terrorist and other politically or criminally motivated groups. That could mean the disintegration of society as we know it.

There may still be time over the next two or three years for the nukes to save the Non-Proliferation Treaty. If they act quickly and sensibly to live up to all their Treaty obligations and provide credible security assurances to the non-nukes, the nuclear powers might be able to create the incentives necessary to attract new parties to the NPT and to avoid withdrawals by some of the present parties. Unless the nuclear powers do so, there will be little likelihood and no point in holding another Review Conference in 1980.

The benefits of saying YES!

The writer acknowledges his debt to Freeman Dyson for pointing out the deleterious effects of eco-freaks and neo-Luddites on technological innovation and scientific creativity.

Paul R. Ehrlich

The scientific community certainly owes a great debt to Freeman Dyson for his analysis of "The Hidden Cost of Saying No!" (*Bulletin*, June 1975). It seems to me, however, that his pioneering effort should be deepened and expanded at a time when the gallop of technology (which has brought the good life to almost a quarter of the people of the world) seems threatened by various doomsayers, eco-freaks, and neo-Luddites.

As an instrument and multi-engine rated pilot, I can testify to the stultifying effect that killing the SST has had on innovation in American aviation. It is clear to me, for instance, that cancelling the SST has had the same kind of dreadful consequences for aviation as did cancelling the Convair XFY-1 VTOL fighter in the mid-1950s. Professor Dyson is much too kind to the opponents of the SST. He mentions only the argument of its possible adverse effect on the balance of payments (which may well have been the one that moved Congress) and neglects to exorcise those who, for instance, seem to have a sentimental attachment to the ozone layer. Just think, destroying much of that layer would substantially weaken the stratospheric temperature inversion and

permit mixing of air pollutants into several times the volume of atmosphere now available to them—an enormous boon to industry as it attempts to maximize the planetary entropy. And we could, of course, alter the global weather patterns, bringing on the rapid, unpredictable changes that Dyson correctly points out might make more of Earth's surface arable.

Professor Dyson did not bore his readers with the detail that any rapid change in climate would probably be a disaster for agriculture and lead to hundreds of millions or perhaps billions of deaths, but some Pollyanna is sure to raise the point. As a population biologist, I can assure the reader that if *Homo sapiens* were killed back to 2 billion individuals by a rapid but ultimately favorable change, the survivors could easily expand the population up to a new carrying capacity of, say, 8 billion in less than a century. Of course, if the climate became semi-permanently unfavorable, we might be stuck with a carrying capacity of 2 billion for awhile. At any rate, a pessimist estimate of the cost of the SST and related technological innovation would probably be no more than 2 billion lives.

It is a pity that Dyson did not expand his SST example further to

include the ozone scare. If, for instance, mankind decided it was better not to attempt to carry on in an environment reminiscent of a toilet seat under an ultraviolet sterilizer, then a vast area for innovation would be opened up for designing a physical or chemical sunscreen for the planet and installing it. And think what a lot of jobs would be created for out-of-work physicists and engineers!

The sunscreen project may go forward anyway, since it now appears possible that another triumph of technology—the fluorocarbon aerosol propellant—will destroy the ozone for us even without the SST. This possibility demonstrates once and for all the vacuousness of the arguments of the SST opponents. Without the ozone, the opponents are only left with silly quibbles. They can claim that it isn't equitable to generate sonic booms that destroy the peace and quiet of hundreds of millions of people, damage their property, and risk the lives of surgical patients in order to allow a few rich businessmen to suffer severe jet lag. Or they might nitpick that SSTs

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would waste huge amounts of fuel (a resource any economist knows is infinite), or that their contrails might cause unpleasant changes in the weather (which we now see would be a benefit rather than a cost).

Yes, as I fly a noisy, inefficient twin-engine aircraft, attempting to cope with an overly complex and confusing air-traffic control system and undependable electronics, or when I travel in airliners in which visibility from the cockpit is highly restricted, airliners in which leaking toilets may cause engines to drop off or in which control cable routing may be fatally flawed, I often think about how cancelling the SST has removed opportunities for innovation in aviation. It is a tribute to the brainwashing power of the eco-freaks that *no one* in my wide circle of acquaintances in aviation has realized that the SST fiasco is the reason that problems besetting aviation today are not being solved innovatively. Doesn't *everyone* understand that the only way to *progress* in aviation is to fly higher and faster?

Perhaps my greatest pleasure in Dyson's article was reading his put down of my well-meaning but confused colleague Professor Paul Berg. Professor Berg and his coworkers worried that their research was going in a direction that could possibly lead to grave difficulties for mankind. They, like many other naive biologists, have been concerned not only about their specific hybrid DNA experiments but also about the long-range consequences of genetic engineering in general. This is in part because an accident in certain types of genetic engineering experiments (*not* those of immediate concern to the Berg group) could kill millions. The possibility cannot be ruled out that a virus could be constructed that would kill *everyone*. But I am sure Dyson would agree that killing everyone would not be a good idea since that would tend to dampen the opportunities for innovation.

Professor Berg and his colleagues, who I will call the Concerned Biologists, have really done the unthinkable. It was a sad day for science—indeed for all professions—when they sat down as an international

group and agreed on voluntary safeguards and restrictions on their kind of genetic engineering research. The voice of reason, calling for *laissez-faire*, was barely audible in the chorus of concern about possible risks to workers in laboratories, the public at large, and the ecosystems of the planet.

Where will all this lead? If professionals start to allow concern for the welfare of mankind to intrude on the fundamental value of in-group loyalty and the attitude that "nobody can tell me I can't do an experiment," then the end is indeed near. As scientists, we must learn to bear the risks we create for others stoically. Already there are enough atavists claiming that society should have a say in risk-benefit analysis. The Concerned Biologists are worried that poorly informed politicians will themselves attempt to place restraints on research, unenforceable restrictions that will inhibit progress by responsible and ethical scientists and provide little or no control over

**Some lack the vision
to see what the world would
be like without thermonuclear
weapons, aerosol deoderants and plastic bread.**

the unethical and incompetent. Surely we must convince these misguided biologists that 'stonewalling,' concealing risks from the public, and denying the need for self-policing—the classic strategy of all professionals—is the only way to avoid interference with our God-given rights to conduct whatever experiments we please. The Concerned Biologists have opened the door to these suppressors of innovation by seemingly admitting that some scientific endeavors carry risks that outweigh their potential benefits.

I think the problem of the Concerned Biologists is readily diagnosed: they lack moral fiber. Contrast the Concerned Biologists' position with that of the team of bomb-test physicists at Los Alamos in July

1945. Those physicists knew the threat of atomic bomb development by the Axis was over, and yet they had put together a very innovative device. They thought there was some chance that exploding a nuclear bomb would detonate the atmosphere, but a calculation showed that chance to be very small. So they went ahead. Would the Concerned Biologists have gone ahead and taken that tiny risk of wiping out life on the planet? Would they have had the foresight and guts to decide to take a chance with the lives of every living creature, including all of their fellow men? Clearly they would not—for they lack the ethical background of those who promote big science as the ultimate human value.¹

And this leads me to my only quarrel with Dyson's article, and a minor one it is. He did not discuss the best of all what-if cases in support of our point of view. What if *all* the physicists of the world had gotten an attack of the kind of scruples shown by the Concerned Biologists and refused to release the power of the atom? Consider what the results of such immoral defection would have been. So many areas of technological innovation would have been damaged or wiped out that the mind boggles. We never would have discovered how large or small a fission bomb could be made or whether a hydrogen bomb was feasible. It seems doubtful that ICBMs ever would have become big business, and all those technicians now struggling to improve SLBMs, MIRVs, MARVs and the like would be unemployed.

Mankind would have been denied a chance to use cheap, abundant power to conquer nature and might have been forced instead to learn to coexist with her. Entire areas of intellectual endeavour, such as deterrence theory and studies of the biological effects of radiation, would have been stifled and, in the absence of the Atomic Energy Commission, mankind today would remain hopelessly ignorant of the limits to which lies, obfuscation, and incompetence can be carried. Perhaps most seriously, we would have discouraged, eager young scientists

in dissident groups or poor nations from finding innovative ways of producing supercriticality from non-weapons-grade plutonium. Can we afford to stifle science among the young and the poor?

The message is clear for all of us. Some people do not understand that technological progress, as we define it, is the greatest good for mankind. Some actually think that a public dampening of the idea that anything that *can* be done *should* be done is good. Some put forward the notion that the negative consequences of many technological advances, such as the release of nuclear energy, far outweigh any possible benefits. Real radicals even claim that mankind would do well to shift its research emphasis away from generating new technological circuses toward the more difficult problems of innovation at the levels of intermediate technology. And traitors to their professions have gone so far as to sug-

gest that voluntary constraints be put on research or, *horribile dictu*, even that the public should have some say in evaluating the risks and benefits emanating from research supported by the public.

These people lack the vision to see what the world would be like without thermonuclear weapons, nerve gases, fast-breeders, smart-bombs, aerosol deodorants, synthetic organic pesticides, supersonic aircraft, uppers, downers, plastic bread and the many other things that make life for almost a billion of us so exciting. Could one even consider life without these and similar wonders truly *human life*?³

Notes

1. We are fortunate indeed that many veterans of the Manhattan project are among the most sturdy supporters of nuclear power (vide the Bethe statement, *Bulletin*, March 1975, pp. 4-5). They are the sort of men who can make rational decisions about future risks and benefits, men fully aware of the technological imperatives that must control human destiny.

2. Those who claim that the Nixon administration would have shown us those limits simply have no grasp of the

duration and quality of the AEC's efforts or the numbers of innovators involved. I need hardly point out that Mr. Nixon had no faulty emergency core cooling system, that his administration admitted that things leaked out, and that even Spiro Agnew was never caught swinging around in a faulty fault tree.

3. Some quibbling anthropologists might claim that, say, the Iking bushmen or the Eskimos led human lives; but this is, of course, confusion on their part. What Iking bushman, for instance, has every had his manhood reinforced by a *Mayaguez* incident? What Eskimo has ever had the capacity to vaporize one of his enemies 57 times over? Neither group uses deodorants. Anthropologists should sharpen their definitions and be more precise. They could learn a lot from physicists like Freeman Dyson, who, for instance, went to the trouble of determining that the putative author of *Our Plundered Planet*, Fairfield Osborn, was in fact Harrison Brown writing under a *nom de plume*.

Acknowledgements

I would like to thank Thomas Andrews, Wilfred Beckerman, Hans Bethe, Werner von Braun, Colin Clark, Buckminster Fuller, Philip Handler, John Maddox, Viscount Pirrie, Dixy Lee Ray, Edward Teller, and Alvin Weinberg, whose thinking on technology has been a constant inspiration to me in the writing of this paper.—P. E.

REBUTTAL

The impact of high voltage lines

Harold N. Scherer, Jr.

The December 1974 issue of your magazine contained an article by Louise B. Young and H. Peyton Young entitled "Pollution by Electrical Transmission." While electrical transmission may be of interest to a number of your readers, the biased and inaccurate article they prepared can in no way advance either the cause of science or of public information.

Over the last several years, the Youngs have staked out extreme positions in opposing transmission line projects. Their methodology consists of combing the literature for any statements which might be favorable to their cause. These are often quoted out of context with some literary license added. Such an approach is unlikely to be accepted by

Harold N. Scherer, Jr., is vice president, Electrical Engineering, of the American Electric Power Service Corporation, New York, N.Y.

How safe are high energy electrical transmission lines? An industry spokesman takes issue with a Bulletin article and says current designs are based on careful studies; the author replies.

those interested in scientific facts rather than partisan speculation.

Many of the inaccuracies and distortions in the article are detailed in a critique which was prepared by a member of my staff, B.J. Ware. The reporting of the Johns Hopkins study of the health of linemen is particularly biased in implying that the study was restricted to sperm counts. It would not have taken much time for your editors to have consulted the full report on this work accurately referenced by the Youngs, and to have noted the conclusion that the health of the linemen was not affected by their work environment. The quoted

results of other studies are similarly distorted.

We believe you owe both your readers and the electric utility industry some further discussion on the subject. Contrary to the writers' implications, substantial research was undertaken to predict the impact of high voltage lines well before the lines were constructed. In fact, the actual line designs were based upon these studies.

The following critique was prepared by B. J. Ware, Head, Electrical Research Section, American Electric Power:

1. *Office of Telecommunications Policy Report*. In introducing the article, the Youngs apparently quote from the White House OTP report of May 1974. This is incorrect. The quotation used does appear in a March 1973 OTP report which has no reference to electrical power lines.

2. *Ozone.* The article states that the problem of air pollution due to chemical reactions in the corona discharge has been treated in some depth in Louise B. Young's book, *Power Over People*. This is incorrect as the book makes no mention of the numerous scientific measurements which have shown no incremental ozone due to power lines.

3. *Electrical Field Strengths.* The statement is made that a potential difference of 50 kilovolts can exist between 2 points of 1 meter apart under a 765 kilovolt line. It is implied that these points are close to ground level. The maximum field strength close to ground level is 10 to 12 kilovolts per meter, approximately, and field strengths of 50 kilovolts per meter can occur only within a radial distance of 2 meters from the conductors.

4. *Fluorescent Tubes.* It is indeed correct that a fluorescent tube can light due to the electric fields associated with transmission lines. However, the threshold level for this to happen is of the order of 400 volts per meter. Such a field strength can be associated with 34 kilovolt lines, some of which have been in service since the last century.

It is also possible to light fluorescent tubes near the ignition systems of automobiles and lawn mowers as well as by rubbing the tube with a piece of cloth. In all these cases the level of illumination produced is less than 1 tenth of 1 percent of what the tube would produce in normal service.

5. *Johns Hopkins Research on Mice.* The article acknowledges that this research was done in "strong electric fields." To put this in perspective, it should be pointed out that the field strength used in the experiments was 160 kilovolts per meter which is approximately 15 times the level to which any member of the public is likely to be exposed. No consideration is given to the stated objective of making the study relevant to the practical problem of exposure encountered by workmen in the electrical industry.

In discussing the results, the Young article mentions only negative aspects and omits the finding that no changes occurred in the organs and tissues of the exposed animals and that there was no effect on their ability to reproduce.

6. *Johns Hopkins Research on Line-men.* The article is particularly biased on this subject, giving the impression that the research was devoted only to evaluating sperm counts. The main conclusion of the paper was that the health of the men was not changed in any way by their exposure to high voltage lines. The study included complete physical exam-

nations and laboratory surveys and included consultations with an ophthalmologist, an otolaryngologist, a urologist, and a neuropsychiatrist.

7. *Soviet Study and Practice.* The article places emphasis on a rather fragmentary report of Soviet research which appears to give different results to those obtained by researchers at Johns Hopkins University and elsewhere in the free world. U.S. engineers and medical personnel have made many efforts to establish a dialogue with their Soviet colleagues on this research. These have been fruitless to date. There are major unanswered questions regarding the Soviets' definition and use of control groups.

For example, the results quoted by the Youngs are based on comparative medical evaluations of two groups who were exposed to different field strengths who were also performing different physical activities. It is pertinent to remember that the extensive 500 kilovolt system in the Soviet Union has field strengths near ground level which exceed those of U.S. 765 kilovolt systems. They impose no restrictions on the use of the land under these lines even for farm vehicles. The Soviet 750 kilovolt lines have field strengths at least 50 percent higher than their 500 kilovolt lines. They have not kept this information secret.

At the 1972 American Power Conference the Soviets presented sufficient data to allow anyone interested to calculate the field strengths associated with their 750 kilovolt and 1,150 kilovolt designs. These data were confirmed at the recent U.S.-USSR technical exchange meeting in Washington held under State Department auspices. At the same meeting they stated that their studies relating to the 4 day per week, 8 hour per day, work situation of substation maintenance workers have no significance to the occasional short-term exposures which might be experienced by the farming community and the general public.

The article takes issue with the fact that the U.S. power companies report ground level field strengths whereas the Soviet literature reports levels at positions more appropriate for the situations of people riding horses or tractors. It does not require advanced mathematical expertise to determine that the electric field strengths due to a line some 13 or 15 meters above ground are almost the same at ground level and at 2 meters above ground.

8. *Project Sanguine.* There is no reason to believe that the Young article is any less biased on this subject. For example, the "very small electromagnetic

fields," which might disrupt the operation of pacemakers are not defined. In actual fact the interference threshold as documented in their reference note 11 was found to be 1 gauss, a level substantially greater than what exists near ground level in the vicinity of any practical power line even when it was carrying its maximum load.

9. *Responsibility.* In listing the various agencies contacted by them in 1970, the Youngs omitted to state their specific interest. A new 765 kilovolt line was planned for construction across their property in southern Ohio. They made substantial efforts to have this line rerouted and failing that to maximize their financial compensation. It is also conceivable that the lack of response from the agencies they contacted might have been due to the unsupported nature of their allegations. At that time they put major emphasis on their theory of substantial ozone pollution due to transmission lines. Since then scientific measurements appear to have caused them to minimize their emphasis in this area and to maximize some of their other allegations.

10. *Alternatives.* The article contained no discussion of alternate methods of transporting energy. In view of the substantial costs required to build transmission lines, the electric power utilities are very reluctant to embark on such projects unless a clear need exists. Given such a need, and the Youngs' dislike of power lines, it would have been appropriate to have included a discussion of alternatives such as the shipment of coal for use in power plants close to the load centers.

Rhetoric won't solve the safety problem

Louise B. Young

Although the representatives of American Electric Power Company have had six months since the publication of our article to comb the article itself and all the references for examples of inaccuracy, distortion, and 'use of literary license,' they have not been able to produce more than one small example—and in trying to make a case for their other allegations they have committed many inaccuracies and distortions themselves.

1. The one error they uncovered is the incorrect date on the OTP report from which the quotation was taken, an inad-

verrant mistake to which I plead guilty. However the mention by OTP of power lines as having potential biological effects is correctly cited and dated. The unwary reader could interpret Ware's statement to mean that the OTP never mentioned power lines as a possible source of energy pollution.

2. Mr. Ware is incorrect in saying that *Power Over People* does not mention measurements of ozone concentrations in the vicinity of power lines. The first edition of the book described (on page 151) the only measurements of this kind that had been released by the time this book went to press. The updated paperback edition of September 1974 discussed the studies that were published during 1973 (pp. 191-193) and pointed out that these studies—commissioned by American Electric Power—were made under lines that were running at 8-10 percent below rated voltage.

Since corona loss and ozone formation are threshold phenomena, at these reduced levels fair weather ozone formation would be substantially zero. The reports of these and a later study—all published in the *Transactions on Power Apparatus and Systems* (PAS-92, No. 3 and PAS-93, No. 5) by the Institute of Electrical and Electronics Engineers—made no mention of the voltage except to say that measurements were made in the vicinity of operating 765 kilovolt lines. Thus the reader was allowed to make the erroneous assumption that the lines were operating at 765 kilovolts. It seems very surprising that the editor of this technical journal did not notice the absence of any data on line voltage and demand that precise information on this important parameter be included.

An even more flagrant abuse of the privilege of scientific publication occurred in a report by Commonwealth Edison and IIT Research Institute (IEEE *Transactions*, PAS-93, No. 5 (Sept.-Oct. 1974). For 1,489 hours (equivalent to 62 days) the 765 kilovolt line being monitored for ozone was *not energized at all*. These facts came to our attention and we pointed them out in the discussion notes which follow the report cited above (PAS 93, p. 1279). The authors admitted that these readings should have been treated as ambient readings.

3. In challenging our figures for electric field gradients, Ware made the incorrect assumption that we were describing a completely unperturbed field. An undisturbed field has only theoretical interest. The minute a person enters it, the field is perturbed and the voltage gradients at the height of a man's head when he is standing in the field of a 765 kilovolt line can be as high as 150



Author Young and her two fluorescent bulbs, lit by the field from a nearby 765-kv transmission line.

kilovolts. The 50 kilovolts we used is actually a very conservative figure. A man on horseback or on a tractor is in an even more highly perturbed and more intense field, not the theoretical unperturbed fields which Ware is citing when he says that they are almost the same at ground level and at 2 meters above ground.

4. There seems to be a great difference of opinion in the electric industry about how to deal with the fluorescent bulb experiment. A report by Charles T. Main, Inc., for the Colstrip Montana Project says that an ungrounded fluorescent bulb did not light under a 500 kilovolt line. Now Ware would have us believe that it will light under a 34 kilovolt line! Actually we believe that neither of these statements is accurate. We have found from repeated experiments that an ungrounded bulb lights at about 170 feet from the center of a 765 kilovolt line. Ware's comments about the fields required to light a fluorescent bulb are not adequately defined or documented, but the figures indicate that he is again citing unperturbed fields instead of fields perturbed by the person and fluorescent bulb.

The real point of the fluorescent bulb experiment is to demonstrate simply, in a way that everyone can understand, the fact that there are continuous currents running in all objects exposed to the alternating electrostatic field. It is not intended to represent a quantitative measurement of this effect. There are many better ways of determining the amount of the currents to which people are exposed.

The examples of ignition systems and charging by friction are not comparable effects because they do not cause continuous currents to run in people for significant periods of time.

5. and 6. Both Scherer and Ware allege that our reporting of the studies on mice and linemen was biased because we emphasized the negative findings. But contrary to their statements, we did not imply that the study on linemen was restricted to sperm count. We specifically reported that "the general health of these men was checked throughout a nine year period with seven medical examinations."

This study and the one on mice were conducted in order to see whether there were any adverse psychological effects from exposure to electrostatic fields. Therefore, any adverse findings are of primary importance. Careful scientific procedure would have required that if any adverse effects turned up, further studies should have been conducted to determine the extent and importance of these effects. But, in spite of some negative results from both studies, no other experiments on possible health hazards from electric fields were conducted by the power companies before building extremely high voltage lines. Scherer says: "Contrary to the writers' implications, substantial research was undertaken to predict the impact of high voltage lines well before the lines were constructed." This statement is extremely misleading in the context of this discussion of health implications. The only published studies on impact considerations were estimates of radio and television interference levels and audible noise.

7. Ware characterizes as fragmentary the report on Soviet research, and yet this research is described in five papers now in English translation.

Although he states that the Russian

Louise B. Young is co-author of the article "Pollution by Electrical Transmission," which appeared in the Bulletin last December. H. Peyton Young, the other author, is associate professor of mathematics at City University of New York.

**"A remarkable job
in peeling away the
layers of secrecy...
The academy's biases
and weaknesses are
exposed in stark form."**

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THE BRAIN BANK OF AMERICA

**An Inquiry Into the
Politics of Science
by Philip Boffey**

Introduction by Ralph Nader

"...a very careful analysis of our
defects."
—Philip Handler,
President of the National
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have finally come under rigorous
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"Boffey has had to proceed in L.F.
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most nakedly exposed."
—N.Y. Times Book Review

*In a speech to the Academy's
annual meeting, April 1975,
quoted in *Science*, June 13, 1975.

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lines produce much higher field gradients than our lines, he does not quantify these differences. The field intensity levels mentioned in the Russian reports are higher than the levels measured in the United States, but we believe that a large part of this difference is due to differences in measuring techniques. The Russians measure at the height of a man's head with an instrument held on a short handle, so the field is perturbed by the presence of the man's body and this leads to higher readings than the measuring technique used by U.S. engineers. In addition it is necessary to take into account a fact not mentioned by Ware; U.S. lines carry 60 hertz AC current, and cause 20 percent larger body currents for a given field gradient than the Russian lines which carry 50 hertz current.

8. A magnetic field of one gauss is approximately twice what could be experienced under 765 kilovolt lines, but that does not provide much of a safety factor for people wearing cardiac pacemakers. Furthermore, the 50 microamp body current mentioned by Furman and others as being sufficient to disrupt some types of pacemakers is exceeded in many situations under a 765 kilovolt line where body currents up to 200 microamps have been measured.

9. and 10. Within the space limitations of a magazine article it is not possible to deal with all aspects of high voltage transmission and its impact on people. This is the reason why I wrote a book on the subject. *Power Over People* describes in detail our personal involvement with extremely high voltage lines and the legal reasons why it was necessary to settle finally with the power company.

The implied and unsubstantiated charge that we brought these matters to the attention of government agencies in order to maximize our compensation is confounded by the fact that we have continued to work for this cause long after the settlement was made. In the past three years we have given freely of our time and effort in the hope that these matters will receive proper attention before more and even larger overhead transmission lines impose unnecessary hazards on the public.

As for alternatives, I consider them to be so important that I devoted one chapter of my book to this subject. Scherer and Ware are in a position to do something constructive about developing new and better ways of transmitting power. I hope that they will give a more open-minded and thoughtful consideration to technological improvements than they have given to the problems we addressed in our article.

ENERGY: THE POLICY ISSUES *Edited and with a Foreword by Gary D. Eppen With a Preface by Harold S. Geneen*

Eminent scholars from a number of fields discuss the major themes of American energy policy. Among the topics covered are federal regulation of petroleum prices, the natural gas industry, and ecological abuse in energy production. Published under the auspices of *The Journal of Business*
1975 Cloth TBA
September

NUCLEAR NAVY, 1946-1962

**Richard G. Hewlett
and Francis Duncan**

"Two superb historians, who produced an official history of the Atomic Energy Commission, have written an equally successful history of the development of nuclear power for naval propulsion of submarines and ships. They describe in detail how Admiral Rickover—clearly the hero—built his unique organization and made it work up to 1962, but they also place the story in a wider bureaucratic and technological framework."—*Foreign Affairs*
1974 544 p., illus.
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BOOKS

Taking stock of the brain bank

reviewed by James A. Fay

The Brain Bank of America: An Inquiry into the Politics of Science. Philip Boffey (McGraw-Hill, 1975, 312 pages, \$10.95).

The practice of science is very much an individual affair. Even when scientists team up to run a complex experiment, the outcome is usually as narrowly defined as the product of the lone scientist pursuing his craft in an out-of-the-way laboratory or in a paper-bestrewn office. The accumulation of these patient efforts has resulted in a scientific edifice of unassailable complexity—a forest unseen by the scientific practitioner admiring the beauty of the tree he has so carefully tended. The links between the various scientific specialties are few and weak, so that collaborative research efforts of several disciplines in basic scientific research are seldom needed. Although scientists have an appreciation of the sister disciplines which populate the scientific world, it is more a consequence of training and liberality of scientific view than of practical necessity.

As long as the scientific questions to be answered are those asked by the individual scientist, the forest will grow, tree by tree, in a satisfactory way. But other voices are being heard in this scientific land, which are asking questions of great urgency to society as a whole. Should we build the SST? Should we ban DDT and other persistent pesticides? What food additives are unsafe? Can radioactive wastes be disposed of safely? These are not the usual questions that scientists ask themselves in their day-to-day work nor is the scientific enterprise presently structured to respond to such questions. There are an active few scientists, however, who devote efforts to "public interest" science in their spare time.

In recent years multidisciplinary scientific groups have been formed under various auspices to study questions of this kind. The results of these efforts have been mixed, in part because there has been no successful pattern for conducting such studies and because they have not always attracted individuals with the level of competence and experience re-

The National Academy of Sciences survives close scrutiny; the beginning of a much needed serious analysis of the politics of science in America

quired. Furthermore, the dynamics of such studies are quite different from that of the usual intensive academic research project, and therefore the demands placed upon individual participants' psychic reserves are unusual. Indeed, to the casual observer of academic politics, it is remarkable—if not miraculous—that some of these multidisciplinary studies have resulted in strikingly comprehensive assessments of substantial merit, especially in view of the inherent tendency of academics to build protective disciplinary barriers around their work. (Perhaps this is overcome by the irrefragable desire to perform well before one's intellectual peers.) There is thus some experience that, in a manner not yet well understood, the whole can be made to exceed the sum of its parts.

The national institution which has

been most involved in assessing the state of scientific opinion on public issues having a considerable scientific component is the National Academy of Sciences. Chartered by Congress in 1864, and for years little more than an honorary but prestigious society of scientists, the Academy in recent decades has enlarged and organized scientific assessment efforts through its wholly owned subsidiary, the National Research Council. The size of this effort is remarkable: in 1970 more than 8,000 scientists served on its numerous committees. Because of its Congressional charter the Academy enjoys preferential treatment both by Congress and the Administration; and by virtue of its scientifically eminent members, it has all the protective armor of a sacred cow.

That slayer of sacred cows, Ralph Nader, has sponsored Phil Boffey's expose of the Academy, *The Brain Bank of America*. But it would be misleading to imply that Boffey's study is merely a washing of dirty linen in public. Instead, it is a serious effort to understand the work of the Academy, mainly through a review of selected controversial studies—most of which appear to have been poorly managed. But because the Academy hierarchy has to a man (there are no women) remained uncommunicative regarding the inner workings of the Academy, Boffey has little to say about the power structure and the determinants of how the Academy manages its efforts. Given the scarcity of institutions which can successfully undertake scientific or technological assessment, it would have been of great public benefit to have had an account of the successes and failures of the Academy's management efforts viewed from their perspective.

In an early chapter Boffey explains how little the Academy members contribute to the work of the National Research Council. Their limited number and advanced age make it impossible for their influence to be strongly felt in the

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myriad panels, committees, boards, assemblies, and commissions which carry on the multifarious studies of the Academy. It is the younger, more active, uncanonized scientists who do the work of the Academy, albeit under the watchful eye of the Academy elders strategically sprinkled about the many boards. But Boffey never explains why the prestige of the Academy members rubs off on so many of their less distinguished colleagues who, so to speak, man the oars while the members seldom ever come aboard the ship. Except for fairness, there doesn't appear to be much reason to adopt the reforms in selecting new members of the Academy which Boffey suggests. Nevertheless, his description of the quaint practices of selecting members is revealing, even to academic scientists experienced in the vagaries of dispensing honorific recognition of scientific achievement.

Slipshod Practices

The meat of *The Brain Bank of America* is its recounting of six major studies which illustrate the author's thesis that the Academy has too often served private interests more than the public interest. But these are no Watergate-type episodes involving deliberate deception. Rather, they are the consequences of slipshod management practices involving the inadvertent selection of biased study team members, failure to review reports adequately, permitting sponsors to influence the results of a study, failure to renew committee membership on a regular basis, obstruction of the publishing of reports in a timely manner, and lack of openness to the public and its access to Academy studies. Some of the more flagrant examples occurred in the 1950s and early 1960s when the Academy was undergoing growing pains. In recent years, the Academy has inaugurated procedural reforms to avoid self-serving committees and panels—probably with some effect since Boffey has found few horror stories of very recent vintage.

An example of the mixture of pitfalls and achievements encountered by the Academy can be seen in Boffey's account of its efforts to deal with the environmental effects of the SST. Starting off on the wrong foot, the Academy was sucked into a narrowly conceived study to allay concern about damage from sonic booms at a time when U.S. gov-

ernment agencies were experimenting with such effects on large numbers of unconsenting citizens in Oklahoma City. But what started as an effort to downgrade the environmental effects of sonic booms eventually led to a definitive conclusion as to the impossibility of alleviating the boom or its deleterious effects. Not content with having clearly advanced the state of knowledge on this problem, the SST committee immediately muddied the waters by publishing ambiguous and misleading statements regarding sonic boom damage to structures in a report which provoked a serious controversy among Academy members. This was followed by some remarkably perceptive analyses of possible effects of SST exhaust emissions on the ozone structure of the stratosphere. But before these seminal ideas could be reviewed by a full-fledged Academy study, the SST debate reared its ugly head in Congress and sent the Academy officials scurrying for cover. Like a TV soap opera, there was more than enough glory and blame for all to share.

In the last chapter, "The Academy and the Public Interest," Boffey sets forth a model for the Academy as a practitioner of "science in the public interest." According to this model, the Academy should break "its servant-master relationship to the government agencies and industrial interests which provide [its] financial support." By securing some funds independent of those supplied by its clients, by opening up its committee membership, meetings and reports to public participation, and by initiating controversial studies rather than simply responding to narrowly formulated requests of willing clients, the Academy would avoid repeating its past sins and better serve the public interest. Indeed, the model has many of the attributes of Nader's Center for Study of Responsive Law.

There is little doubt that such reforms would serve to make the Academy more responsive to society's need for the best scientific thinking on thorny problems, and would provide for an open style of operation which is more consonant with today's participatory politics. The Academy is not a scientific Vatican and President Philip Handler is not its Pope; the time has passed for the scientific saints to come marching in. The public still possesses a great respect for science and scientists, but suspects someone behind the scenes is calling the tune played by

our scientific pipers. The great scientific prestige of the Academy—and it is very great indeed—cannot assuage public suspicions rubbed raw by the incredible domestic political events of the last few years. The case for procedural reforms, as Boffey presents it, is convincing.

Boffey's model of the Academy as a national practitioner of science in the public interest deserves closer examination than he has given it. It is certainly an appealing notion that the very scientists who have advanced the limits of scientific understanding should be called upon to inform the public and its officials about the truth or falsity of the conflicting claims surrounding such issues as the use of atomic materials, pesticides, drugs and food additives, or the misuse of the natural environment, to name a few. A vociferous few have indeed raised public fears regarding such matters (much to the displeasure of their colleagues); but the vast majority happily pursue their work in the laboratory and library untroubled by the winds of worry and the clash of debate. But the role of public interest scientist is not a congenial one for most scientists, for it raises many procedural issues which are beyond their ordinary experience, such as the nature of the adversary process, the definition of the public participation in public policy choices.

There are many examples of how scientists (usually self-chosen) have functioned very effectively in this manner outside the aegis of institutions such as the Academy (an excellent account has been given by Joel Primack and Frank von Hippel in *Advice and Dissent: Scientists in the Political Arena*), but it is questionable that the special talent for doing so is widely distributed among our best scientists. Yet there are enough instances of Academy successes—even in Boffey's book—to support the view that within a proper institutional framework (as yet but dimly understood) even those 8,000 scientists working voluntarily for the National Research Council could produce results which would warm the cockles of Ralph Nader's heart.

Boffey's book is lively but fair in its treatment of the Academy's embarrassing episodes. It is a beginning to some serious analysis of the politics of science which is very much needed. It should not be regarded as a feather-ruffling exercise but as an invitation to a further exploration of the public role of science in America.

Atoms

The Nuclear Fuel Cycle: A Survey of the Public Health, Environmental, and National Security Effects of Nuclear Power

prepared by the Union of Concerned Scientists

The official U. S. Atomic Energy Commission (AEC) assessment of the hazards associated with the nuclear fuel cycle appeared in 1972. The Union of Concerned Scientists (UCS) felt that the AEC document was marked by serious errors and omissions—for example, the official report did not even touch on the problems of the accidental release of radioactivity or of safeguarding nuclear materials from clandestine weapons makers—and for this reason, UCS initiated an independent, parallel study of the subject. *The Nuclear Fuel Cycle* is its report.

Separate chapters review the exposure of uranium miners to radioactive, lung-cancer-inducing gas; the milling of uranium and subsequent disposal of radioactive tailings; the mechanisms and consequences of catastrophic accidents; the problems of diversion of nuclear materials for terrorist purposes; hazards of transport, in particular the possibility of the release of radioactive cesium; the history of operations of the first U. S. fuel reprocessing plant; and storage/disposal of high-level radioactive wastes. In addition, Daniel F. Ford, Henry W. Kendall, and James J. MacKenzie have contributed a chapter that overviews the entire cycle and its hazards.

This book is a revision and enlargement of a report originally published by Friends of the Earth for UCS.

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for War and Peace

Citizen Groups and the Nuclear Power Controversy:

Uses of Scientific and Technological Information

by Steven Ebbin and Raphael Kasper

"A study of citizen-group participation in Atomic Energy Commission hearings on nuclear power plants concludes that the 'whole process as it now stands is nothing more than a charade.'"

"The outcome, a victory for the utility sponsoring the nuclear generating station, 'is for all intents and purposes predetermined,' the study finds.

"The authors are Steven Ebbin, a 41-year-old political scientist who is director of the Environmental Policy Study Group at George Washington University here, and Raphael Kasper, a 31-year-old nuclear engineer on the staff of the National Academy of Sciences. Their study was financed by the National Science Foundation. . . .

"Their appraisal of the motives, methods and effectiveness of citizen groups that challenge nuclear power projects is regarded as the most comprehensive such inquiry made."

—Edward Cowan, *New York Times News Service*

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Proliferation and Pollution

Publications prepared by the Stockholm International Peace Research Institute (SIPRI)

Some—but not all—SIPRI publications are available from The MIT Press.

Those available through 1975 are as follows:

World Armaments and Disarmaments:

SIPRI Yearbook 1974

\$20.00

World Armaments and Disarmaments:

SIPRI Yearbook 1975

\$25.00

Tactical and Strategic Antisubmarine Warfare

by Kosta Tsipis

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